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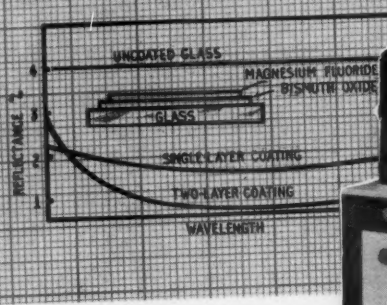

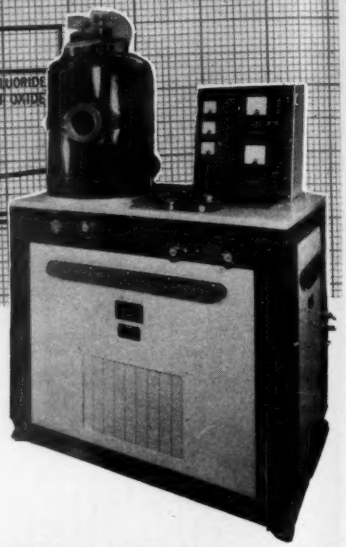

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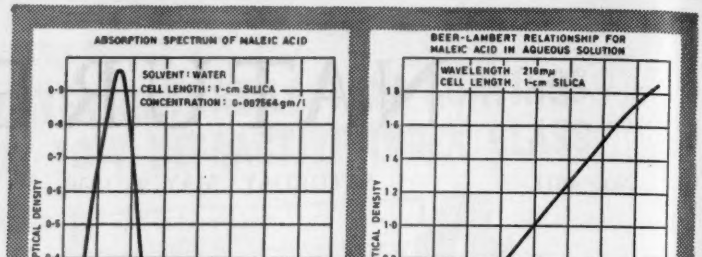

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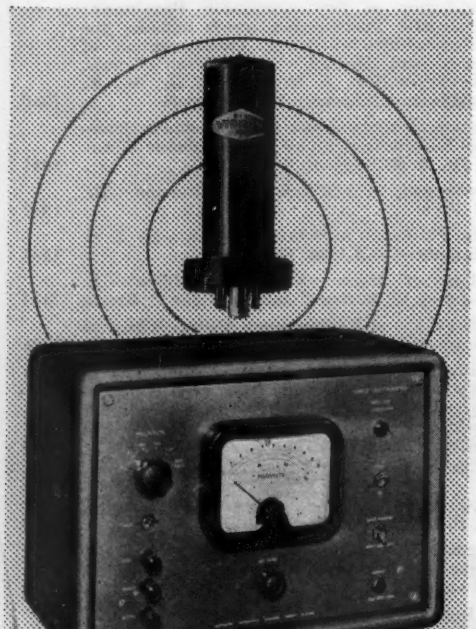

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LIBRARY SERVICES IN BRITAIN

IN its report on "University Development, 1952–1957", the University Grants Committee pointed out that much university expenditure for research is expenditure on libraries, but although it is possibly true that libraries are relatively more important to the arts and related subjects than to the sciences—it has been estimated that library expenditure per arts student is double that on other students—the importance of efficient and adequate library service in science and technology is not easily over-stressed. The University Grants Committee did not discuss library expenditure exclusively in relation to research in the humanities and the social sciences; but in this report, although it recognized the constantly increasing pressure on university libraries, it did not emphasize the importance of the library itself as a factor in university education in general. Library expenditure increased by 64 per cent between 1951–52 and 1956–57, but is still only 3·9 per cent of university expenditure compared with 3·8 per cent in 1951–52, whereas full-time students have increased from 83,458 to 89,866 and academic staffs from 8,952 to 10,485; by 1961–62 the number of students is now expected to rise to 110,000 or more. Readers' places in university libraries increased by 17 per cent between 1953–54 and 1956–57, from 17,895 to 20,938; but the increase is unevenly spread, and in some universities the position is getting worse. Moreover, the 37 per cent increase in the cost of books, periodicals and bindings between 1951–52 and 1955–56 has reduced the real increase in library expenditure to something less than 10 per cent.

Although the University Grants Committee reports that capital works to cost about £1·75 million have been started in seven universities in Britain, there is no note of real urgency in its report. According to F. E. Hirsch, the Association of College and Research Libraries now recommends that the library should receive 5–7 per cent of the total educational budget of an institute. At the Home Universities Conference last December, Prof. R. J. Pumphrey advocated doubling the library allocation and instituting a three-year plan with the object of guaranteeing to every undergraduate access at very short notice to any book he is likely to want and a place in which to read it as one means of meeting the pressure which the forthcoming expansion is likely to put upon the universities. It may be doubted whether doubling the present expenditure of £1,620,978 is anything like enough to achieve this object, and Prof. Pumphrey gave no indication of the capital expenditure that would be involved. To provide libraries of 40,000 volumes for every 600 students, with a further 10,000 for every 200 students up to, say, 300,000 volumes, as suggested by the Association of College and Research Libraries, is a major task demanding financial resources which neither the University Grants Committee nor the Ministry of Education or local authorities appear at present to be contemplating. Indeed, the expansion of university libraries, within the limits of any foreseeable resources that are likely to be made available, may need planning with some regard to the requirements of the colleges of technology and the technical colleges, if only from the point of view of the very close co-operation demanded by the Association of College and Research Libraries Committee on Standards. All are affected, too, by the presence of students coming from homes where there is no tradition of books but rather Ruskin's "abyss of penury", and who have not learned the value of possessing and cherishing even a few well-chosen books, and are unprepared to make the sacrifice that such purchase may entail.

In practice, it is probably only within the framework of an efficient and reasonable regional public library system that the needs of even the university student are likely to be met from the resources which the universities will be able to devote to library services within the next decade. These services, as already indicated, have to meet the needs of research workers, quite apart from the invaluable contribution which the universities themselves have long made to the national lending library system in Britain. The universities must thus be vitally interested in any proposals for the development of a national reference library or a national lending library for science and technology, just as the scope and content of any such libraries should be planned with some regard to regional needs and resources if duplication and waste of effort and resources are to be avoided.

It is in accordance with this that Miss H. Mews, of the University of the Witwatersrand, in a paper at the International Conference on Scientific Information last November, argued that it is a first responsibility of any central government to set up a central scientific and technical library and a national scientific and technical information service, either as an independent entity or attached to some existing institution. This has already been recognized as a primary need by the United Nations Educational, Scientific and Cultural Organization, which assisted, for example, the organization of the scientific documentation centre, Insdoc, in India. In the Union of South Africa, the Library and Information Division of the Council for Scientific and Industrial Research was from the start planned on broader lines than as a library service to the Council's laboratories only, while the Federation of Central Africa is at present considering grafting a similar service on to the library of the University College of Rhodesia or one of the existing scientific research institutes.

To a certain extent, the Department of Scientific and Industrial Research, which has been made responsible for the new National Lending Library for Science and Technology, now being planned at Thorp Arch, Boston Spa, Yorks, has, since a recommendation to that effect was endorsed by the Advisory Council on Scientific Policy some five years ago, been developing its Information Division to help

inquirers over the whole field of science, apart from the military sphere. In an address on the work of the Department which he delivered to the Society of Chemical Industry at Exeter last year, Sir Harry Melville laid some stress on this function of the Department which, he said, it is proposed to extend further. In view of the success of the Department's regional offices in Edinburgh and in Wales, it was thought desirable to extend the facility over Great Britain; for although the staffs in these offices could not be experts on everything, they could provide the links whereby information could be obtained very quickly. Dr. W. L. Francis stated on September 24 that further centres already exist at Manchester, Birmingham and Newcastle upon Tyne, and others are being planned where possible in co-operation with other libraries, universities, technical colleges, chambers of commerce, etc.

There would appear to be some danger here of overlapping with the work of the Association of Special Libraries and Information Bureaux, which for more than thirty years has been functioning effectively in precisely this field; but apart from this, the success of the development will depend largely on the calibre of the regional officers. Sir Harry Melville's address, however, added little to the very meagre accounts of the work of the Intelligence Division of the Department which have hitherto appeared in the Department's annual reports; some comprehensive survey of its activities and its resources for that work appears to be overdue, particularly in view of concern currently entertained as to the ability of the Council of Scientific and Industrial Research to secure adequate resources for its work as a whole. That the position is, however, improving is indicated by a written answer in the House of Commons on March 25, when Mr. H. Nicholls, Parliamentary Secretary to the Ministry of Works, as representing the Lord President of the Council, stated that provision has been made in the five-year plan for 1959–64 of the Department of Scientific and Industrial Research for the National Lending Library for Science and Technology to be fully operational in 1962. It is anticipated, he said, that some buildings of the former Royal Ordnance Factory, Thorp Arch, which are being adapted for use by the library, will be ready for occupation by March 1961. When the library is fully operational, it is estimated that the annual cost will be £280,000, and the estimated cost of adapting and equipping the buildings for the library is £250,000.

It is now clear that the new Lending Library is to be independent of the Science Museum Library, although it will take over some of the literature held by the latter, which in future will concentrate on meeting the growing needs of the Imperial College of Science and Technology, London. Apart from the additional cost to the new library involved in this decision, it does not appear that attention has been given to the possibilities of savings both in the purchase of stock and in storage which would follow from the planning of a lending library in some relation to a reference library. For a library which it is intended should function primarily for postal loans, there may be no inherent need to stock its holdings of periodicals other than as microcards, at least if the original periodicals are readily obtainable from the reference collection for photocopying. It appears, moreover, from the location now announced, that the idea that the new lending library should also serve industrial areas of the north of England and the Midlands as a scientific reference library has been abandoned. Dr. D. J. Urquhardt expressed the opinion at Washington that perhaps 75 per cent of the current serials in the Science Library are so little used that one loan copy in the United Kingdom would meet the needs of all users. His statement that about 1,250 serials are sufficient to meet 80 per cent of the demand, if confirmed, has a vital bearing on the economics of regional development and the inter-library lending and stocking that are involved.

There is yet another important aspect to which neither Sir Harry Melville nor the Advisory Council on Scientific Policy refers, and that is the relation of these schemes to the British Museum Library. According to the last report of the Advisory Council on Scientific Policy, the idea of a Science Centre has now been definitely abandoned; but it should be remembered that part of that original scheme involved making the British Museum Library the repository for literature more than fifty years old. No statement has yet been made as to the scope of the new Science Reference Library, but Dr. Urquhardt has indicated that the new Lending Library will only be comprehensive from 1950 onwards. There is a possible gap here to be considered if the national library service is to be as vital and positive as Mr. F. C. Francis recommended in his presidential address to the annual conference of the Association of Special Libraries and Information Bureaux at Nottingham last September.

Mr. Francis said he hopes that in the next four years it will be possible to lay the foundations of a new library service which will give the British Museum the place in the modern world which it occupied at the end of the nineteenth and the beginning of this century. Much that he said in this address is highly pertinent to the development of national library services primarily intended to serve science and technology. He recognized, for example, the inevitability of some decentralization, but he was forthright as to the danger of being conservative and parsimonious in our thinking about libraries. That is what is most likely to prevent their full and aggressive use, and depreciate a real national asset. Mr. Francis believes that the staff of the British Museum Library are able to play a full part in co-ordinating library activities, provided there is an appropriate breakdown of services and sufficient awareness of what was being done in other libraries. He made the pertinent point that special librarians owe it to their clients to be familiar with the resources of the great collections, and ready to take the trouble to find the relevant material.

Reiterating the importance both of adequate financial resources and of seeing that money is not

wasted, Mr. Francis remarked again that, with the present growth of literature and the present demands made on libraries, no library can look upon itself as being self-sufficient. It is in this context that it is essential to keep to the minimum the number of institutions receiving in full, for example, government publications, with the considerable cataloguing and storage problems involved. Again, the National Library is the only library with an obligation to maintain and preserve its collections, and this responsibility would be difficult or impossible to distribute. Likewise, it is often the only institution in a country which can provide detailed information on the thousand and one subjects for which no specialized library exists.

The Intelligence Division of the Department of Scientific and Industrial Research thus must take full cognizance of the scope of the British Museum Library if it is to perform efficiently the functions now entrusted to it; and that will be even more the case as its responsibilities for providing scientific advice for our embassies through scientific attachés increase. Sir Harry Melville and Dr. W. L. Francis referred specifically to this function, and the importance of the scientific attaché was also emphasized by Miss Mews, who referred to the value of the two-way flow of information and co-operation which is promoted by the system of scientific attachés and scientific liaison officers. Miss Mews mentioned particularly its bearing on the co-operation of professional and scientific societies, and the potentialities of their co-operation in a national library service appear to have been ignored almost completely in the official approach to the new schemes.

The original Science Centre scheme was designed to facilitate the co-operation of such bodies, with full recognition that their participation offered substantial advantages in economy, both of storage and holdings and in operating costs. It is unlikely in view of the decisions which some of the scientific societies have already been forced to take in regard to library and other accommodation that any Government scheme could to-day obtain the full advantages of co-operation that were possible ten years or more ago; none the less, even informal co-operation could offer very real benefits. This was fully realized by President Eisenhower's Science Advisory Committee, and when that Committee's Panel on Scientific Information reported last December, stress was laid on the value of the contribution to the dissemination of scientific information generally made by scientists and technologists in addition to their regular scientific or technical work. Accordingly, the scheme proposed by the Panel is intended to enable the National Science Foundation to take full advantage of all such initiative, whether informally or formally, and to create no more than the minimum of formal organization. What that minimum is cannot be determined either for the United Kingdom or for the United States without a clear view of the whole field and the potential contribution which a wide range of institutions and professional bodies could make—and this appears to be notably lacking in Britain. Moreover, the responsibilities of the National Science Foundation in regard to science information, which, besides research, includes the support of scientific publications, put it in a much stronger position to influence development and provide missing services than any existing institution in Britain could yet hope to do.

What is most discouraging is the absence of any realization at high level of the importance of an adequate national library service or appreciation of the basis on which it must be built. The Committee appointed by the Minister of Education in September 1957 under the chairmanship of Sir Sydney Roberts was concerned with the structure of the public library service in England and Wales and was charged with advising what changes, if any, should be made in administrative arrangements, having regard to the relation of public libraries to other libraries. It might have been expected, therefore, that the Committee would have given some attention to these broader issues, particularly in view of the place which some of the commercial and technical libraries at, for example, Manchester, Sheffield, Liverpool, Leeds and elsewhere have already taken as regional centres. This development, however, which was so well described by Mr. J. P. Lamb four years ago in his book, "Commercial and Technical Libraries", is virtually ignored in the Committee's report*, as are also the earlier report of the Library Association on the post-war development of the university and research libraries of Great Britain, the Association's proposals for the post-war re-organization and development of the public library service and the McColvin report on which the proposals were based. The Committee recognizes, indeed, that co-operation between libraries is essential, and that existing arrangements embrace all public libraries in the country, as well as many university libraries and those of learned societies, scientific and professional organizations and industrial concerns. It is convinced that the regional library systems must be put on a firmer basis and that both the regional bureaux and the National Central Library should be provided with sufficient financial resources.

Nevertheless, it rejects the suggestion that the regional systems and the National Central Library should form integral parts of one national system, the cost of which would be met jointly by the central Government and by the local authorities and other sources, the contributions to be assessed by a new national committee on library co-operation. Instead, it would place the responsibility for providing a satisfactory system of library co-operation on the existing eight regional library bureaux, financed by compulsory payments from each library authority. Virtually, the sole positive contribution to more effective library co-operation proposed by the Committee is the recommendation that under a new Public Libraries Act all library authorities should be given specific powers to combine or co-operate with

* Ministry of Education. The Structure of the Public Library Service in England and Wales: Report of the Committee appointed by the Minister of Education in September 1957. Pp. iv+57. (Cmnd. 660.) (London: H.M. Stationery Office, 1959.) 3s. 6d. net.

each other and to co-operate with education and other authorities.

This would indeed remove existing legal obstacles, and the responsibility for seeing that inter-library co-operation is effective would rest ultimately with the Minister of Education, who should, in the Committee's view, exercise a general oversight of the public library service. This is in keeping with the educational functions of the public library service, but is not enough. The real problems in establishing an adequate national library service which will meet scientific and technical needs, among others, have not been faced; and the extent to which it is a factor in industrial and scientific efficiency and not merely in education is not understood. The contribution which the commercial or technical library of a large local authority could make in developing an economic service is ignored, as are the financial implications which arise when such a body is asked to meet national needs from local resources. If the nation's growing needs for scientific and technical information, educationally or in research, in industry and in commerce, are to be met at any reasonable and practicable cost, full and effective account must be taken of all existing resources, and the means provided for efficient co-operation without making demands liable to impair the efficient discharge of any institution's primary responsibilities. Further, we must proceed boldly and imaginatively to fill lacunæ in the existing structure from national resources, making full use of all appropriate advances in the handling and processing of scientific and technical or other information.

MORE PROTEIN CHEMISTRY

Advances in Protein Chemistry, Vol. 12

Edited by C. B. Anfinsen, Jr., M. L. Anson, Kenneth Bailey and John T. Edsall. Pp. x+687. (New York: Academic Press, Inc.; London: Academic Books, Ltd., 1957.) 14 dollars.

THIS addition to the well-known series on "Advances" contains seven articles, each written by masters well versed in the difficult art of weaving together several hundred disconnected papers into a readable whole. The activation of the zymogens, reviewed by Neurath, and the conversion of fibrinogen to fibrin, reviewed by Scheraga and Laskowski, are chemically related phenomena. Each involves the activation of a precursor by the hydrolytic action of a specific enzyme. It is only in the conversion of trypsinogen to trypsin, however, that the process has been fully elucidated, while the activation of chymotrypsinogen, procarboxylase, pepsinogen and fibrinogen each consists of a different and complex proteolysis as yet only partly understood. In fibrinogen, moreover, the splitting by thrombin is followed by a polymerization, the intricacies of which challenge the physical chemist. The discussion of these phenomena becomes highly technical, but both articles contain lucid summaries for the non-specialist.

Literature on the chemical nature of antibodies is a jungle of confusing observations through which Isliker guides us with skill. Their purification, and the complexities of even the simplest immune reaction by whole animals, baffle the biochemist. Decisive advances probably await the production of antibodies by homogeneous cultures of specific cells or, even better, by microsomal particles extracted from them.

Li's short article reviews the structure and biological activity of the melanocyte-stimulating hormones, including the most recent results by himself and Harris on the relations between these hormones and adrenocorticotrophic hormone. Work on prolactin, an interesting protein of molecular weight 24,000 with a cystine bridge screening the terminal carboxyl group, is also described.

The abnormal hæmoglobins are a new field to which Itano has made outstanding contributions; his paper gives a lucid summary of the observations and a thoughtful discussion of the physiological, chemical and genetic problems they raise. I should have thought it an understatement, though, to describe Ingram's work on the chemical differences between normal and abnormal hæmoglobins as a "significant advance".

"Protein Crystallography without Tears" might be the title of Kendrew and Crick's article, a popular exposition spiced with pictures of mermaids and quotations from "Alice in Wonderland". However, the frivolous tone conceals scientific rigour in an introduction to crystallography for the non-physicist. This is followed by a balanced account of X-ray work on fibrous and globular proteins, the latter now fortunately rendered obsolete by Kendrew's three-dimensional Fourier analysis of myoglobin.

Goodman and Kenner's monograph on peptide synthesis describes the newer synthetic methods, followed by more than a hundred pages of tables with data and references for all the peptides ever made.

The book is a mine of useful information and should be on the shelf of every biochemical department.

M. F. Perutz

TEXT-BOOKS OF ZOOLOGY

Borradaile's Manual of Elementary Zoology

Thirteenth edition, revised by W. B. Yapp. Pp. viii+769. (London: Oxford University Press, 1958.) 30*s*. net.

General Zoology

By Mary J. Guthrie and Prof. John M. Anderson. Pp. xv+708. (New York: John Wiley and Sons, Inc.; London: Chapman and Hall, Ltd., 1957.) 60*s*. net.

General Zoology

By Claude A. Villee, Warren F. Walker, Jr., and Frederick E. Smith. Pp. xix+877. (Philadelphia and London: W. B. Saunders Company, 1958.) 52*s*. 6*d*.

THE authors of these books set out to provide a text for a first course in zoology. "Borradaile's Manual" is the thirteenth edition of a standard work which many generations of students found well written and intelligible, and this last revision, coming so soon after the major one in 1955, was in response to criticism concerning illustrations, misprints and errors which then appeared. Guthrie and Anderson's "General Zoology" is planned to replace a standard American work—Curtis and Guthrie's "Textbook of Zoology"—but in preference to a fifth edition an essentially new book has been written which preserves

characters of the old, brings the text up to date and introduces an insight into modern trends of zoology. As in Villee, Walker and Smith's "General Zoology", the authors set out to give a comprehensive understanding of the subject, recognizing that anatomy is a prerequisite to an appreciation of the functioning of organisms.

The last two books deal initially with cell physiology, metabolism and reproduction and give the necessary chemical and physical background. A survey of the invertebrates follows and some comparative anatomy of vertebrates (which receives a broader treatment in the work by Villee, Walker and Smith), and there are sections on ecology, evolution and genetics. This wide approach to the subject, which reflects its shifting emphasis, necessitates the pruning of some detailed morphology, and one wonders whether such groups as Rotifera, Acanthocephala, Bryozoa and Hemichordata are included for any reason other than reference. The sections on embryology are concise, but it would be helpful to have a precise account of the movements of presumptive areas during gastrulation. Many illustrations in these volumes are new, especially prepared for the text, and others adopted from elsewhere are redrawn. All are labelled so that uniformity is maintained. The drawings in Guthrie and Anderson are executed with great care; in Villee, Walker and Smith more diagrammatic representations are favoured, and this is in conformity with differences in the text and the more leisurely style in Guthrie and Anderson. There are excellent photographs of living animals.

It is a pity that "Borradaile's Manual" still impresses one with its traditional lack of functional treatment, but this would perhaps demand alterations greater than the publishers would welcome in the light of the book's undoubted popularity and its present low price. The introductory chapter on animal organization could profitably be expanded to reflect recent advances. The detailed morphology, an essential constituent of the type system (in which the reviewer firmly believes), gives guidance in practical work (though scarcely replaces a practical text), but needs supplementary comparative studies to give a better understanding especially of vertebrate evolution. A large number of illustrations (forty-three) have been replaced, and more replacements are promised. Figures have been compiled from a variety of sources, and unfortunately this is reflected in their labelling, which has not been altered and results in an incongruity of style and information which is distracting to the beginner. V. FRETTER

NATURALLY OCCURRING QUINONES

Naturally Occurring Quinones

By Dr. R. H. Thomson. (Organic Chemistry Monographs.) Pp. vii+302. (London: Butterworths Scientific Publications; New York: Academic Press, Inc., 1957.) 50*s*.; 9 dollars.

THIS interesting class of natural products claimed the attention of organic chemists at an early date on account of the importance of some of its members as dyestuffs; and interest in them has continued ever since and has greatly increased in recent years. They vary in complexity from simple derivatives of benzoquinone through the numerous naphthaquinones and anthraquinones to polycyclic molecules like erythroaphin and hypericin.

In this excellent monograph Dr. Thomson has given a critical account of the chemical investigation of all the natural products that have been isolated, characterized, and identified with fair probability as quinones. He has wisely included a number of compounds that are not in fact quinones, such as the sennosides, barbaloin, and rugulosin, the chemistry of which is so bound up with that of related quinones that their omission would have been rather artificial; on the other hand, he has omitted compounds such as methylene-quinones and phenoxazones which contain quinonoid systems that are not terminated at both ends by carbonyl groups. The great volume of degradative work published on some members of the group is concisely summarized, and the more interesting points in their chemistry, such as the unexpected cyclizations and molecular rearrangements that often puzzled early workers in this field, are fully discussed in the light of more modern results and ideas. The degradations and syntheses described are made easy to follow by the generous provision of structural formulæ. Special care has been bestowed on the listing and nomenclature of the animal and vegetable sources, for which there are separate indexes in addition to the main index. There is an extensive bibliography appended to each chapter with references ranging in age from the Old Testament up to as late as 1957, and there are interesting sidelights on the history and uses of many quinonoid pigments.

This volume is indispensable as a reference book for anyone interested in natural colouring matters, and the reviewer also found it a pleasure to read.

R. I. T. CROMARTIE

AVIATION MEDICINE

Aviation Medicine

Selected Reviews. Edited by Clayton S. White, W. Randolph Lovelace II, and Frederic G. Hirsch. (AGARDograph No. 25.) Pp. vii+305. (London and New York: Pergamon Press, 1958. Published for and on behalf of Advisory Group for Aeronautical Research and Development, North Atlantic Treaty Organization.) 70*s*. net.

THESE reviews summarize some recent developments in analytical techniques and instrumentation of use to research workers in the highly specialized fields embraced by aviation medicine.

The relation of some of the material to aviation medicine is tenuous. A. E. Reif contributes a lengthy chapter on the physical properties of aerosols, some of which is out of place in a book such as this. Similarly, T. L. Chifelle's chapter on technical developments in pathology is not wholly relevant, making no mention of aviation medicine: nor does Chifelle mention the promising field of aviation pathology.

I. G. Bowen contributes a useful review of techniques for the measurement of atmospheric ozone and outlines a programme for determining whether or not ozone is a hazard. M. A. Palmer summarizes recent developments in high-speed photography, but fails to give an adequate explanation of image fixation on continuous film cameras. J. Clark contributes a short chapter on pressure transducers and Frederic Hirsch has written a useful review of temperature-measuring techniques. Hirsch omits any refer-

ence to radiometry, which perhaps deserves mention, and is mistaken in suggesting that thermocouples can be used to measure absolute zero. B. B. Longwell reviews methods for investigating some of the biochemical and physiological changes which occur during stress.

The remaining chapters are devoted to respiratory physiology.

U.P. Luft surveys the important features of various techniques used in direct spirometry and residual volume determination, but omits bronchospirometry and the measurement of maximum breathing capacity and timed vital capacity.

Clayton S. White has written a comprehensive review which describes the physical methods that have been developed for the analysis of respiratory gases. The paper covers methods based upon the measurement of thermal conductivity, paramagnetism, refractive index, velocity of sound, and absorption, mass and emission spectra. It is in the field of emission spectra that the author has made a significant contribution to this aspect of respiratory gas analysis. This section also contains an extremely useful collection of references. It suffers, however, in that it is not sufficiently critical and an investigator new to the field receives little guidance as to which is the best method to employ in given circumstances.

In a further paper written in collaboration with W. R. Lovelace II, White discusses in detail the use of the nitrogen meter. This section is full of intensely practical details with regard to the calibration, accuracy and use of this instrument. In the opinion of the reviewer it should be read by anyone who proposes to use this important tool of the respiratory physiologist.

The final paper, by N. P. V. Lundgren, presents a comparison of the Rahn-Otis sampler with a method for obtaining end-tidal air from the oropharynx. He does not consider, however, the generally accepted limitation of end-tidal air sampling as a method of determining the composition of the alveolar air during quiet breathing, when the volume of alveolar air is inadequate to wash out the respiratory dead space.

All the reviews in this book are by American authors. Most of the material is of great value to the specialist in aviation medicine. Some of the material, excellent in itself, would have more impact in a different publication.

BIOLOGICAL INFORMATION: THEORY AND PRACTICE

The Biological Replication of Macromolecules
Edited by F. K. Sanders. (Symposia of the Society for Experimental Biology, No. 12.) Pp. vi+255. (Cambridge: At the University Press, 1958. Published for the Company of Biologists on behalf of the Society for Experimental Biology.) 50s. net.

AT the present moment considerable interest is centred around the mechanisms involved in the transfer of intelligence in biological systems, particularly with respect to the relationship which is believed to exist between the structure of two types of large molecules found in all living organisms, namely the proteins, which were originally regarded as the fundamental molecules, and the nucleic acids, which are now thought by many to be the repository of all the information required by an organism. Unfortunately, although this relationship is probably one of the most important in biology, experimental evidence for it is somewhat slight and may even be conflicting. Thus, for example, several enzyme systems are now known which will produce reasonable facsimiles both of ribonucleic acid and deoxyribonucleic acid from simple substrates, but there does not appear to be any necessity for a specific coding system for this synthesis, nor do the nucleic acids produced appear to have any particular information content built in. Indeed, such systems will willingly accept substrates which they do not normally encounter, and incorporate them into the polymers.

So far as the proteins are concerned the situation is rather different, because *in vitro* systems do not accomplish much more than incorporate radioactivity from individual labelled amino-acids into material of large molecular size, and they appear to be rather restricted in their synthetic capabilities.

However, in spite of the general paucity of experimental evidence, there is a great deal of effort now being expended in attempts to elucidate the type of code which could relate the structures of proteins to those of the nucleic acids, using methods of the kind employed in cryptographic analysis. Apart from the fact that new constituents of nucleic acids are now being discovered at regular intervals, and that work on small polypeptides has shown the existence of structures so far undetected in proteins, the problem is complicated further by lack of information about polynucleotide sequences, and by the fact that the proteins themselves are coded by a complex transposition code, which relates their catalytic surface structures to the residue sequences in their polypeptide chains. Consequently, it is necesary when engaging in this entertaining pastime to simplify the problems to such an extent as to render the conclusions doubtfully significant.

It was probably with intent to discuss the present implications of this type of work that the Society for Experimental Biology decided to organize a symposium on "The Biological Replication of Macromolecules" which has since appeared in book form. The precise meaning of the rather indefinite term macromolecule seems to have eluded the organizers, and the various contributors interpret it as meaning anything from a small polynucleotide to a nucleus, with a few proteins and polysaccharides thrown in for good measure. The general tendency of the contributors has, however, been to discuss the more complex types of structure, with the result that the emphasis is largely on genetical rather than on biochemical phenomena. Outstanding among these papers is an account of the conjugation process in *Escherichia coli K*12, while other papers discuss hereditary mechanisms of bacteria, and phenomena occurring on tumour tissue transplantation and during virus replication.

On the simpler level, there are accounts of remarkably successful attempts to isolate individual nucleic acids by ion exchange, and several papers on protein synthesis including an authoritative exposition of the rules of 'coding'. The general impression given by these papers is that a lot of experimental work is still necessary before the results can be interpreted.

It need scarcely be said that all the articles are of a high standard, and it is regrettable that a volume containing such a wealth of information should not contain an index.

ROY MARKHAM

MUSICAL ACOUSTICS

A MEETING of the Acoustics Group of the Physical Society, under the chairmanship of Mr. T. Somerville, was held at the Royal Academy of Music on February 26 to discuss: "Of what Use is Acoustics to the Musician?". Prof. E. G. Richardson opened the proceedings by introducing a list of possible uses of acoustic knowledge and apparatus, as follows.

To obtain an accurate tuning note. This is an obvious use for an electronic oscillator, amplifier and loud-speaker. The conventional instruments for giving the key-note (in singing) or *A* (in playing) are the tuning-fork or the pitch-pipe or the note given by a wind instrument, usually the oboe in the orchestra. The last two suffer from temperature uncertainties, though there is some advantage in having the oboe giving the *A* since the pitch reached by the player after warming up, even if not precisely 440 vibrations per second, will be one to which all the wind players can conform.

To check the pitch of a recorded performance and of the correct intonation of the different parts in concerted music. This is an interesting pastime which tells one quite a lot, perhaps too much. By its means it is possible to check the intonation and pitch discrimination of a solo instrumentalist or singer during a performance. It also brings out quite prodigious feats of tuning and re-orientation of pitch on the part of a good orchestra. Thus, as was shown by an analysis made of a Dutch tape-recording, at a certain performance of Beethoven's first pianoforte concerto the piano was at 440, to which the orchestra tuned within 1 c./s. When accompanying the soloist the orchestra accommodated itself to his pitch, but rose 2 c./s. higher in the *tutti*. On the other hand, a rather regrettable state of affairs was disclosed in a broadcast of Bach's oboe concerto. Oboe and orchestra tuned to 440, but when the piano came in it was at 435. It says much for the string players that they overcame this handicap by playing at 439 (to which the oboe had dropped) when accompanying the solo, and at 435 in the ensembles with piano.

The pitch generally tends to rise above the tuning note during *forte* passages, probably because the notes of the wind instruments tend to rise in pitch with the blowing pressure. The tests also showed—what has been long known—that the human voice at its best is by no means so precise in maintaining constancy of pitch as the mechanical instruments.

The fact is that a musician may be most meticulous about notes being in tune at one instant and will tolerate a considerable degree of mistuning at others. It is natural that he should wish the notes of prominent diatonic chords in a composition to be in tune, but care less about passing notes, which may be elided rapidly, or the little unaccented grace notes which the composer writes as a semi-tone above or below a prominent chord note, probably with no intention that they should be rendered as semi-tones. It is also natural that singers asked to descend a semi-tone and return to the original note, in a type of cadence prominent in the sixteenth century, should make the drop less than a semi-tone in anticipation of rising again. It is a question of accent and speed.

To establish a succession of notes to form musical scales. The construction of a musical scale stretching from one note to the other and the historical evolution of scales seems to me a rather arid and arithmetical subject of study.

Whether our musical sense is an occidental or an oriental one, we all seem to agree that the scale shall cover an octave (though it is true that, in modal counterpoint, the 'stretch' was regarded on occasion as exceeding the octave). Also, rather surprisingly, we all seem to divide the octave of the scale into seven steps whether these are based as in Western music on tones and half tones, or whether, as in Eastern music, some of the intervals are third or quarter tones. In physics, we are mostly Pythagoreans, that is to say, we think of the octave as a length of stretched string which is sub-divided into shorter pieces of string bearing fixed ratios to the whole, but throughout the ages there has existed a type of musicologist who conceives—to me—more complicated sub-divisions, which he advocates as *the* perfect scale.

For most of us two scales suffice, the major and minor of just temperament. Formerly, as is well known, there were six scales in Western music, all having two semi-tone intervals placed at different points in the scale and rejoicing in the lovely names of Dorian, Phrygian, Lydian, Mixolydian, Aeolian, Ionian (thinking in terms of the pianoforte we can say that these had as key-note in turn *D*, *E*, *F*, *G*, *A* and *C*, but were entirely made up of the white notes). Of these, that based on the note *C* became the major and that on *A* the minor scale. Later, when composers learnt to harmonize, they wanted to make a composition modulate into other keys, and although this was possible for singers and for players on the viols and sackbuts of the epoch, it was not so on a wind instrument with side holes, or on instruments with keyboards, because the frequencies of some notes on the new tonic (key-note) were not equal to any of those of the old key. So a compromise had to be reached, and after various trials the equi-tempered scale, in which the semi-tone is equal to the twelfth root of two, received the support of Bach (who wrote for it his "48 Preludes and Fugues") and became established.

An orchestra or a choir then, accompanied on a keyboard instrument, had to follow equal temperament. Singers, who in common with string players and trombonists have the pitch of the tones they produce variable at will, often claim that when singing unaccompanied (*a capella*) they use the untempered scales. But they are not justified by pitch analyses which have been made using tape records in the way already mentioned. Hard facts show that both they and unaccompanied string quartets use a temperament which is mid-way between the just and the equi-tempered. It is, of course, possible to hear music played on keyboard instruments in just temperament, in a limited number of keys. Such instruments have red and green as well as black and white keys, but require an expert to play on them.

To provide a scientific basis for theories of harmony. It is impossible in a short space of time to give a complete history of the ideas of consonance and dissonance of intervals. It is well known that Helmholtz gave the physicist's and physiologist's view of the basis of dissonance. To him it was a question of the relative number of beats between two tones

sounded together as a chord. The more frequent these, the greater dissonance. He made a graph showing the number of beats for each interval of the major scale. But to a musician these matters are not so simple. In fact, some of the ideas of Helmholtz on this question of discords have since been discredited.

In classical music the striking of a discord is rather like the tasting of wine or of sauce. The ear, like the palate, has to be prepared for it; it is then savoured and it should be followed by some contrasting sweeter sensation.

This was how it was with discords in the classical period of harmony. At first only the octave and the fifth were regarded as concords. It is supposed that the other intervals evolved in the theory of harmony on the basis of a series of upward jumps of a fifth and downward jumps of a fourth. In this way the major and minor thirds appeared and also the seventh. At first the fourth was regarded as an imperfect interval, but by the fifteenth century the root tone, third and fifth formed the accepted concord. Only when the third above the fifth was added to form the seventh above the root of the chord was a discord felt, and this discord had to be prepared by first appearing in a concord and resolved on the following chord in a special way. Later, other thirds were added above the seventh to give the ninth and the eleventh.

It was Bach who found that the discord could be made more piquant by not preparing it, and since his time a composer has followed his whim in this matter. In turn, the necessity for resolution has ceased to be felt by composers. Wagner did not resolve ninths, and Ravel treated elevenths as consonances.

To me the necessity for resolution and not the counting of beats is the essence of a discord. If I do not feel, for example, the seventh on the fifth note of the scale to lead on naturally to the chord on the key-note or any other concord, that chord is free in its progression and so becomes for me a concord.

To measure the acoustic spectra of musical instruments in order to study and improve functioning. Perhaps this is the most rewarding service which the scientist can perform. There exist firms in the musical instrument trade, perhaps more numerous in the United States and Germany than in Britain, which are well aware of the possibilities of acoustic spectrometers and employ people competent to operate them. Adjustments are then made, for example, in the thickness of the belly of a stringed instrument, in the metal of an organ pipe, so that the 'voicer', or his counterpart outside the organ-building trade, can observe the effect on the formant of the instrument which in turn governs the spectrum and the efficiency of the instrument. Not only are such studies carried out by employees of firms, but physicists who are also music amateurs have spent a lifetime of leisure in such work. One need only refer to what Dr. F. A. Saunders has done for the violin and viola in Massachusetts and Dr. W. Lottermoser for the organ (and certain other instruments) in Eastern Germany.

To make experiments with new spectra (on electronic musical instruments). The advent of the electronic organ has given harmonic innovators a very useful tool to try experiments on, though they do not always realize that the loud-speaker of the instrument can superpose its own formant on the register in which the player elects to play his new-fangled music. This new electronic composing is not very popular in Britain, but there exist flourishing groups of its exponents in Germany, Italy and Switzerland.

To obtain design data on concert halls and music rooms. Perhaps in this matter, the musician especially realizes his debt to the scientist who works in acoustics. Auditorium acoustics must remain largely subjective, and musicians reserve their right to say when a hall is good for the performance of music—the difficulty is that all do not speak with one voice on such topics. Nevertheless, when composers and performers are able to say what they want—in reverberation, relative weight to different parts of the gamut and so forth—the acoustic consultant is usually able to advise the architect so that a concert hall or music room satisfactory to its users results.

Dr. B. W. Robinson, continuing the discussion, said that most musicians got on quite happily without studying acoustics. Also, he thought that not many instrument makers used the precision electronic apparatus now on the market for calibrating their products. Prof. Richardson had, however, omitted to emphasize the importance to a musician of the latest high-fidelity reproducing equipment, which was a great boon to an artiste wishing to judge, after listening to himself, to what extent his performance had improved. Self-recording was also of use in teaching elocution, and for singers to show them how bad their pitch discrimination often is.

Mr. D. M. A. Mercer said that acoustic testing with modern equipment could teach the craftsmen tricks not in the text-books and enabled the organ builder to determine how many stops would be required on an organ design for a new church. By its means, Dr. R. W. Young in America had been able to formulate the best tuning system for a pianoforte. This was never done exactly in accordance with the equi-tempered scale.

Dr. A. E. Brown said that he found a knowledge of acoustics helpful in learning to play a musical instrument, for example, in forming the best embouchure for the French horn.

Mr. A. Rakowski referred to the importance for music of having people trained to take full responsibility, from the artistic point of view, for making musical recordings. In the present state of perfection of sound technique in disk recording, radio and film, it is possible for recorded music to achieve a very high degree of artistic value, but people who work in the technique should have a knowledge of both acoustics and music. In the Music Academies of Warsaw and Detmold, special courses have been established for training such personnel.

Dr. H. D. Parbrook thought that there was too much of the music student's time devoted to theoretical study of scales and tuning. Not enough attention was paid to the tonal qualities of electronic musical instruments.

Mr. Nightingale also emphasized the importance of the tape recorder to the virtuoso to improve his playing.

Dr. H. Lowery recalled Sir Hubert Parry's dictum that the acoustical knowledge of a student varied inversely as his knowledge of music. He thought there still was too big a gap between the teaching of acoustics and the requirements of the musician. He said that an understanding of auditorium acoustics was most important to the composer. It was because Bach had an instinctive knowledge of this that his works were so well adapted to the baroque style of building which was prevalent in his day.

E. G. Richardson

VAN ALLEN RADIATION OF SOLAR ORIGIN

By Prof. PAUL J. KELLOGG
University of Minnesota, Minneapolis

A NUMBER of authors[1] have proposed that the radiation discovered by Van Allen and co-workers[2] results from the decay of neutrons produced in the Earth's atmosphere by cosmic rays. There is some doubt, however, as to whether the low-energy (< 1 MeV.) radiation near the Earth, which on this hypothesis would be due to the β-decay electrons from thermal neutrons[3], is not too intense to be attributed to this source. Although the albedo source is not yet completely ruled out, one should consider other sources for the Van Allen radiation.

The Van Allen radiation is observed to be divided into two zones, and it has been suggested that the inner zone results from neutron albedo, while the outer zone is due to solar particles. This suggestion may be right, but does not help if the inner zone radiation is too intense to be accounted for on the albedo hypothesis. Nevertheless, streams of solar particles are very likely candidates as sources of Van Allen radiation and it is necessary to consider them in more detail.

The picture of the Van Allen radiation which we shall present is rather similar to that published by Gold[4]. We suppose with Gold that part of the Van Allen radiation has its origin in streams of solar gas which somehow become trapped in the Earth's magnetic field. The mechanism for this trapping is not understood, but it is reasonable that, since the pressure of the Earth's magnetic field varies as r^{-6}, most of the gas is injected at a rather constant distance from the Earth. The particles of the gas diffuse away from their radius of injection, but the mechanism for diffusion which we shall present is different from that suggested by Gold.

It is a well-known result of the perturbation theory of the motion of a particle in a slowly varying magnetic field that the magnetic moment of the particle due to its helical motion is a constant of the motion to a high degree of accuracy. Our diffusion mechanism maintains the constancy of $W_\perp/B$, so that particles which diffuse toward the Earth into regions of stronger magnetic field must also acquire energy. We discuss this acceleration in detail below. As the particles diffuse upward and downward they may also be lost to space or into the Earth's atmosphere.

This picture might explain the presence of the two Van Allen zones. With Gold, we assume that injection usually occurs at 5–7 Earth radii. Thereafter, the particles diffuse inward and outward, those going outward being lost in a short time. If the diffusion is very rapid in the region beyond 4 Earth radii and considerably slower in inner regions, then the position of the maximum density will move inward as the diffusion proceeds. We discuss below a possible reason for faster diffusion beyond a few Earth radii. It seems reasonable to suppose that the injected particles have an energy spectrum such that the bulk of the particles have too low an energy to be recorded by the instruments used by Van Allen (~ 15 MeV. threshold for protons) but that some few have sufficient energy. Then we expect that the counting-rate will be high near the Earth, where many of the particles have been accelerated to high energy, and high near the injection point, where there are very many particles, but may be low in between, where many particles have been lost and too few have been accelerated to energies above the detector threshold to make up the deficit. In this picture, of course, the total density of particles in the part of the Van Allen radiation of solar origin continually decreases inward from the region of injection and the dip in counting-rate between the two zones is only an effect of the detector threshold.

In order to illustrate the mechanism for acceleration of the particles in the Van Allen radiation, we consider first the following simple case. Consider a system with a constant electric field in the x-direction and a magnetic field in the z-direction which varies in the y-direction, and consider charged particles moving in the $z = 0$ plane. The constancy of the magnetic moment to high accuracy for this case has been demonstrated by Kruskal[5]. According to the Alfvén perturbation theory[6] of motion of particles the radiation of gyration of which is small compared to the scale of field variation, the electric field causes the particle to drift in the y-direction, with velocity v_y. The gradient in B_z causes the particle to drift in the x-direction with velocity v_x. Here:

$$v_y = -c\frac{E_x}{B_z} \qquad v_x = -\frac{c}{e}\frac{W}{B_z^2}\frac{\mathrm{d}B_z}{\mathrm{d}y} \tag{1}$$

where W is the energy of the particle. The gradient in the magnetic field, therefore, carries the particles into regions of higher or lower potential, depending on the signs of E_x, B_z and $\mathrm{d}B_z/\mathrm{d}y$. Since the total energy is constant, the kinetic energy will be decreased or increased. Rather extreme examples of these orbits are illustrated in Fig. 1.

Now we consider the motion of particles trapped in the field of the Earth. As is well known[4], the particles have a very fast gyration around the lines of force, a somewhat slower back-and-forth motion along the lines of force, and a still slower precession around the Earth due to the gradient of the Earth's field. It is this last motion which is of importance here. Since the Earth's field is somewhat irregular, the gradient will cause the particle to precess faster at some longitudes, slower at others. We consider two sources of electric field. First, during a magnetic storm the changing magnetic field of the Earth will produce electric fields. Secondly, various collective oscillations, in particular flute oscillations, may produce electric fields.

We cannot discuss flute oscillations properly at the present time, because we do not understand them. The criterion given by Longmire and Rosenbluth[7] for flute instability, namely, that the magnetic field confining the plasma be convex outward, cannot

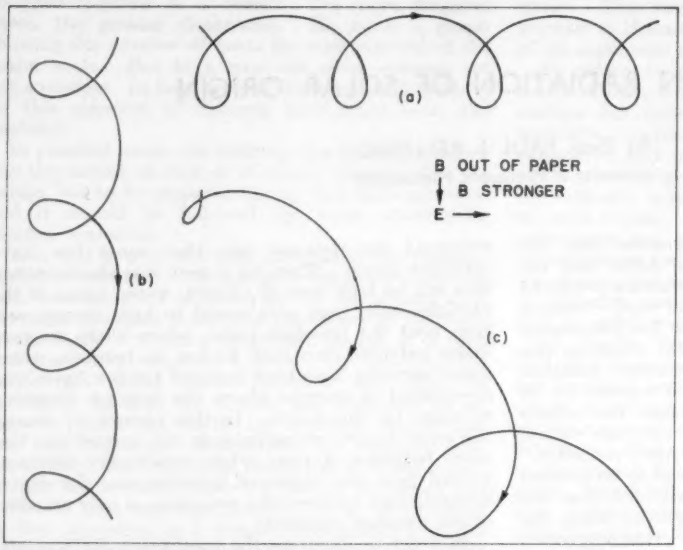

Fig. 1. Typical particle orbits: (*a*) $E = 0$, drift produced by gradient of B; (*b*) $dB/dy = 0$, drift produced by E; (*c*) combined effect of E and dB/dy

be correct, or the Van Allen zone would not be able to trap any radiation; it would be flute-unstable at its upper boundary. It can be shown that neither the conductivity of the background gas, nor 'thermal' dispersion of the particles which would smear out the flute oscillations before they could develop, is sufficient to prevent growth of instabilities at distances less than about 100 Earth radii. Longmire and Rosenbluth neglect the change in energy of the magnetic field, which seems incorrect since the energy of the field is reduced by a quantity of the same order as the plasma energy, and it is probably this neglect which makes their treatment inapplicable in the present case. Intuitively, we might expect that a magnetic field would be rigid enough to confine a very low density of plasma, and that the condition for flute instability should involve the ratio of plasma energy density to field energy density. We may therefore speculate that flute instabilities may be important in the outer parts of the Van Allen radiation, where the field is weak, may account for the approximate r^{-4} decrease of its intensity, and may produce a much faster diffusion in this region.

Both these electric fields have zero time average, so that if the Earth's magnetic field had rotational symmetry they would not produce any net change of particle energy. However, in conjunction with the space-varying gradient of the Earth's field, acceleration and deceleration may be produced. For example, suppose the electric field has a positive value E for a time t and a negative value $-E$ for the same length of time. While the electric field is positive the magnetic field gradient will carry the particle through a potential difference eEv_1t, and while it is negative the particle will be carried through a potential difference $-eEv_2t$, where v_1 and v_2 are average precessional velocities produced by the gradient of the Earth's field at the appropriate longitudes. If $v_1 > v_2$ then the particle will have a net gain in kinetic energy:

$$eE(v_1 - v_2)t \qquad (2)$$

and vice versa. The magnetic moment of the particle, $W_\perp/B$, will be constant, and so if the particle gains energy it will move into regions of stronger field, and thus nearer the Earth, and vice versa. Thus, the electric fields of magnetic storms coupled with the irregularities in the Earth's field will produce a diffusion of particles inward and outward and at the same time an acceleration of those particles which happen to go inward.

The orbits of the particles can be discussed in more detail using, in addition to the constancy of the magnetic moment, the constancy of the integral $J = \int v_\parallel dl$ taken between turning points[7]. We present one result only. As the particles diffuse, their turning points move on lines which are roughly radial.

The use of these two constants is the major difference between the present treatment and that of Gold. Gold assumes that irregularities in the field, or electric fields, separately, can produce diffusion. This can only take place if at least one of J and $W_\perp/B$ is not conserved. In a dipole field, the orbit of the particle's guiding centre can be specified by giving three parameters, for example, the energy of the particle, its pitch angle at the equator, and the distance from the Earth at the equator. If in addition to J and $W_\perp/B$ the energy of the particle is constant, as it would be in the case of static magnetic fields of whatever irregularity or of slowly varying electric fields the time average of which is zero, then the particle cannot diffuse but follows a prescribed orbit, the three parameters of which are determined by the three constants of the motion.

Gold's treatment implies that J is not constant. Unfortunately, no estimates exist for the rate of change of J, and so the efficacy of his diffusion mechanism cannot be estimated.

We now make a very rough estimate of the rate of diffusion. Since Van Allen particles do not approach nearer than 1,000 km. to the Earth, they will not be affected by irregularities in the Earth's magnetic field of scale much less than 1,000 km. We may expect, then, that the principal irregularity will be that produced by the Earth's octupole moments, which contribute about 10 per cent of the Earth's surface field. The wave-length of the field is one-third of the Earth's circumference, so that particles which precess around the Earth will be most strongly affected by those Fourier (time) components of the electric field which have the same period as the time the particle takes to precess one-third of the distance around the Earth. This time is given by a modification of equation (2):

$$\tau = \frac{1}{3}\frac{2\pi R}{\frac{c}{e}\frac{W}{B^2}\nabla_\perp B} \approx 10^4\left(\frac{R_E}{R}\right)\left(\frac{1}{W}\right) \qquad (3)$$

where W is the energy of the particle in MeV. and τ is in seconds.

During magnetic storms the magnetic field-changes have periods of a fraction of a minute to a few hours.

Only particles of energy greater than 1 MeV. are, therefore, able to take advantage of these changes. For particles of lower energy a different approximation must be used. We take $300\gamma = 3 \times 10^{-3}$ gauss as a typical change of the magnetic field in this time and $\dot{E}$ approximately equal to $R\mathrm{d}B/c\mathrm{d}t$

$$\approx 3\,\frac{R}{c}\,\frac{10^{-3}\ \text{gauss}}{\tau}$$

The change in energy during one cycle of the electric field is given by equation (2). Since B varies by 10 per cent at the surface and decreases as $1/R^3$ compared to the dipole part, we may expect $(v_1 - v_2)$ to be of the order of $\frac{v}{10}\,(R_E/R)^3$ and vt to be one-third of the orbit circumference. During one cycle, then, we estimate:

$$\Delta W = eE\,\frac{vt}{10} = W\left(\frac{R}{B}\,\frac{\mathrm{d}B}{\mathrm{d}z}\right)\left(\frac{3\times10^{-4}\ \text{gauss}}{B}\right)\left(\frac{R_E}{R}\right)^2 \quad (4)$$

$$\approx W(10^{-3})\,\frac{R}{R_E}$$

Over a long period, T, since this is a diffusion phenomenon, the root-mean-square (r.m.s.) change $\Delta W/W$ will be proportional to $\sqrt{T/\tau}$. Here T is counted only during magnetic storms. We estimate that magnetic storms are going on 0·1 of the time, and so:

$$\frac{\Delta W_{\text{r.m.s.}}}{W} = 10^{-3}\,\frac{R}{R_E}\,\sqrt{\frac{T}{10\tau}} \quad (5)$$

where T is now total elapsed time. The time (in seconds) for the particle to make a change of a factor of 2 in its energy is of the order of:

$$T_2 = 10^7\left(\frac{R_E}{R}\right)^2\tau = 10^{11}\left(\frac{R_E}{R}\right)^3\left(\frac{1}{W}\right) \quad (6)$$

with W in MeV.

This estimate is very rough and is certainly not to be trusted to within a factor of 10. As it stands, however, it gives a diffusion time of 10^9 sec. for a 10-MeV. particle at two Earth radii, which is about 1,000 times shorter than the scattering time for protons, and comparable with that for electrons. The estimate is not valid for particles of energy less than 1 MeV., but an appropriate approximation in that case indicates a diffusion time of the order of 10^{11} to 10^{13} sec., which is somewhat longer than the scattering life-time of either protons or electrons. The diffusion is therefore not capable of supplying low-energy particles to the inner Van Allen zone, as they will be scattered down into the Earth's atmosphere before diffusing very far. Of course, low-energy particles of solar origin might still be supplied directly to the inner zone during times of low-latitude auroræ.

In our opinion, the outlook for Gold's diffusion mechanism is similarly pessimistic. The rates of change of the two near constants of the motion, $W_\perp/B$ and J, are probably very rapidly varying functions of the particle momentum, since the asymptotic expansion, for $W_\perp/B$ at least, vanishes to all orders[5]. The intensity of high-energy particles shows that they are trapped for a long time, so diffuse fairly slowly; therefore, the low-energy particles probably diffuse even more slowly than by the mechanism considered here. We are therefore left with neutron albedo, low-latitude-auroral streams and, possibly, high-altitude nuclear explosions, as the possible sources of the low-energy Van Allen radiation in the inner zone.

I wish to thank Prof. T. Gold for helpful discussions concerning both the present article and his own theory of the Van Allen radiation, and also Profs. E. P. Ney and J. R. Winckler.

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A NEW THEORY OF THE SOLAR CORONA

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In the following discussion we will consider the experimental facts which are known about the solar corona and show that these facts are consistent with a model of the corona quite different from that currently accepted.

According to the present model, the solar corona consists of a gas of electrons and protons with a small mixture of heavy elements which is isothermal at a temperature of approximately a million degrees and which is in hydrostatic equilibrium in the gravitational field of the Sun. The alternative model which we propose is that the solar corona consists of trapped charged particles moving in the magnetic fields of the Sun, very like the charged-particle cloud surrounding the Earth which has recently been discovered by Van Allen[1] and his collaborators.

Although hydromagnetic effects in the solar corona have been discussed (see, for example, Alfvén[2]), the idea of the corona as a trapped charged particle assembly has, to our knowledge, not been proposed.

The solar corona is usually said to be very much hotter (10^6 deg.) than the bodies in contact with it (6,000° for the Sun, 100° for the outer boundary). It is not known what supplies its energy, but probably the heating is a hydromagnetic effect produced by the turbulent motion in the outer part of the Sun, or by 'granulation waves', as suggested by Schwarzschild[3]. There is no general agreement as to

the details of this heating mechanism, nor as to the particle energy spectrum which it produces. A lower limit to the supply of energy to the corona may be easily set by computing known losses of energy such as radiation in the observable regions and evaporation from the outer corona. Van de Hulst[4] estimates that the whole energy of the corona must be supplied every 10^8 sec. This time is so short that many features of the corona cannot be expected to be in thermal equilibrium. For example, the collision time for an electron at one solar radius above the surface, where the density is 10^6 electrons/cm.^{3}, is of the order of 10^2 sec. for a 100-eV. electron but is about 10^5 sec. for a 3-kV. electron. Therefore, if the heating mechanism accelerates some electrons to energies greater than 3 kV., these electrons may have energy supplied faster than they lose it by collisions. We may expect, therefore, that in small regions of the corona those electrons having energies in the vicinity of 100 eV. have approximately a Maxwellian distribution of energies, but there may very well be a very long tail of high-energy particles on this distribution. Since the time for exchange of energy between heavy ions and electrons is much longer than that for particles of the same mass, we may expect that the electrons may have a different 'temperature' from the ions, where now temperature refers only to the Maxwellian distributed part of the energy spectrum.

It is likely that the corona contains magnetic fields of 1–10 gauss so that a 100-eV. electron has a radius of gyration of about 10 cm. In 10^8 sec. and with a collision time of 100 sec. such electrons diffuse only a negligible distance. We cannot, therefore, expect that the corona is in anything like equilibrium with a distribution given by the Boltzmann factor $\exp(-MG_m/RKT)$.

Deviations from thermal equilibrium are, in fact, required by the observations. The high temperature, or more properly energy content, of the corona is deduced from: (1) the scale height of the corona; (2) ratios between the populations of ionization states of heavy atoms; (3) Doppler broadening of emission lines; (4) measurements of radio intensity; and (5) Doppler broadening of the Fraunhofer lines. All these measurements give different temperatures (except 5, which gives only a lower limit). We can understand this if the different processes are affected to a different degree by the lack of thermal equilibrium.

In the following we will discuss the properties of the corona listed above in terms of their present interpretation together with their explanation in terms of our suggestion. In addition, we will consider certain observational facts about the corona which are ignored in the current interpretation, but which, however, can be understood on the basis of plasma trapped in a solar magnetic field.

Scale Height of the Corona

Observations of the brightness distribution of the corona during eclipses show that this brightness varies approximately as $1/R^6$, where R is the distance from the centre of the Sun. This experimental fact, first discovered in the eclipse of 1898, is sometimes referred to in the literature as Turner's law. This observation is the basis for the determination of electron densities in the corona on the assumption that the visible light is Thomson-scattered by free electrons. A brightness distribution varying as $1/R^6$ implies an electron density which varies as $1/R^5$ because of the geometry of the problem. Fig. 1 shows a plot of the inferred electron coronal densities and a graph of the expected electron densities for an isothermal atmosphere held by the gravitational field. We have also shown in this figure the profile of intensity of the Van Allen radiation, in units of the Earth radius rather than in units of the solar radius as is done for the corona observation. It can be seen that the principal difference between the observed coronal electron densities and the number of particles in the Van Allen radiation is that the Van Allen radiation has a pronounced maximum where the corona electron density apparently does not. At large distances from the trapping object, however, it appears that the decrease in intensity of the Van Allen radiation is very much like the decrease of electron densities in the corona.

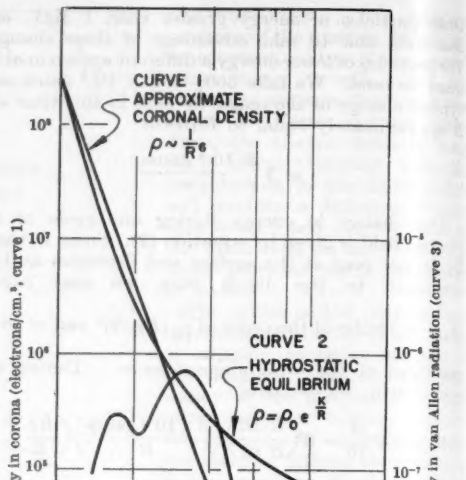

Fig. 1. Plot of electron densities in the corona and Van Allen radiation around the Earth. Curve 1, coronal electron densities vs. radius from the centre of the Sun measured in solar radii; curve 2, an arbitrarily normalized hydrostatic equilibrium curve, such as would apply to the million degree isothermal corona; curve 3 (scale at right), inferred particle densities in the Van Allen radiation (ref. 12) obtained from *Pioneer III* data by multiplying the counting-rates reported by Van Allen and Frank by a factor of 5×10^{-11}. This is equivalent to assuming that the particles measured by them are relativistic particles

Reference to Fig. 1 shows that the particle densities in the solar corona and in the Van Allen belt around the Earth are in approximately the ratio of 10^{12}. If it is assumed that the solar source injects particles

which could be trapped in either field, then the flux, because of the inverse square law, would be 10^4 times stronger in the solar belts than in the terrestrial belts. In order to account for the remainder of the factor 10^{12}, one would have to conclude that only 1 in 10^8 of the solar injected particles can be captured at the position of the Earth, that is, the solar trapping field is extremely effective in constraining particles to the immediate solar environment. Because the Van Allen radiation has a maximum and then decreases with increasing distance from the Earth, it is clear that the Van Allen belt is not just an extension of the solar corona, as it could be if Turner's law held to the orbit of the Earth.

Because of the limitations in observing the Sun's corona at large distances from the Sun and because of the F corona, it is not possible to distinguish by brightness distribution between a power law decrease of the intensity and the function predicted by hydrostatic equilibrium.

In the hydrostatic model of the corona, Turner's law is the experimental fact, and temperature of a million degrees must follow from the consequent scale height, with a mean molecular weight of 1/2.

The similarity of the intensity distribution at large distances of the corona and Van Allen radiation, however, suggest that the same mechanism may be responsible for both. In a crude way, one can, in fact, justify a power law distribution of intensity of the kind indicated by Turner's law. If the trapping of the high-energy particles were produced primarily by a field with dipole components at large radii, this field would be $B = C/R^3$. Since the energy density of the magnetic field is $B^2/8\pi$, the energy density in the magnetic field can be seen to vary as the inverse sixth power of the distance from the centre of the Sun. In magneto-hydrodynamic models one usually assumes that the energy density of the contained particles is some fraction of the energy density of the field. If this were so, and the energy spectrum were unchanging with distance, the number of particles per unit volume would be expected to go as $1/R^6$. It seems quite clear from observations that the Sun's magnetic field is not a simple dipole field, but the possibility of interpreting Turner's law in a theoretical way as indicated above suggests that magnetic trapping in a more complicated field than a dipole field may be the explanation of corona atmosphere.

Ratios between the Population of the Ionization States of Heavy Atoms

Following the discovery by Edlèn that emission lines of highly ionized atoms were present in the spectrum of the corona, the presence of these emission lines was taken as confirmation of a high kinetic temperature. If these emission lines do, indeed, represent the effect of kinetic theory collisions they also imply an extremely constant temperature throughout the solar corona. For example, the ratio of the intensity of the line due to Fe XIV to that due to the line of Fe X should vary by a factor of about 10^5 for a change in temperature of a factor of 2. It is observed that the ratio of the intensities of these lines varies very little with the solar cycle and with position in the corona.

The hydrostatic model takes this to indicate that the temperature of the corona is extremely constant. On our model the constancy of the ratio of these line intensities would arise because the ionization degree depends on the cross-section for ionization by fast electrons in the high-energy tail. Since the atoms would then be ionized by electron impact by an approximately uniform energy spectrum of particles, one would not expect large variations in the intensity ratio of these lines. It should perhaps be pointed out that the ions themselves would be trapped in the magnetic fields and would therefore not reflect hydrostatic equilibrium of the atmosphere.

Doppler Broadening of Emission Lines

The coronal emission lines are observed to have shapes which are accurately those expected for Doppler broadened lines[5]. The corresponding temperatures, however, range from one to four million degrees, so are both higher and more variable than those derived from (3), yet the same atoms give rise to both effects. On our model, the emission lines would give the 'temperature' of the Maxwellian distributed part of the heavy ion energy spectrum. We suppose there are too few ions in the high-energy tail to affect the line shape.

Measurements of Radio Intensity

The radio emission of the Sun at various wavelengths is sometimes considered supporting evidence for the high coronal temperatures assumed in the hydrostatic theory. The apparent temperature which the radio emission indicates for the quiet Sun varies all the way from 10^4 to 10^{10} deg., with the lower measurements actually applying to chromospheric 'temperatures' because of the wave-lengths used. The range of temperature obtained indicates that the phenomena are too complex to understand on a simple isothermal model, but do not contradict our picture.

Doppler Broadening of the Fraunhofer Lines

In the inner corona, Fraunhofer lines are observed with great difficulty or not at all. This is interpreted to be an indication of a very high kinetic temperature of the scattering electrons which leads to Doppler broadening of these lines to such a degree that they can no longer be seen. The Fraunhofer lines appear again in the outer corona, and this is the primary experimental evidence which leads to the introduction, at large distances from the Sun, of an F corona or dust corona composed of large slowly moving particles.

The Doppler broadening differs from the other measurements in that it gives only a lower limit to the temperature. The lower limit is somewhat below 10^6 deg. It is also the one effect which is not strongly influenced by the presence of a moderately small fraction of high-energy electrons. Therefore, the 'temperature' of the Maxwellian distributed part of the electron spectrum may be considerably less than 10^6 deg. This is required by our model in order that the Maxwellian distributed electrons do not produce too high a degree of ionization of the heavy ions.

It can be seen from the foregoing that the principal observational facts about the corona can be equally well, if not better, described by our model than by hydrostatic equilibrium in a gravitational field. There are, in addition, at least three outstanding effects, usually ignored in explaining the solar corona, which have a bearing on the suggestion of magnetic trapping. These are: (*A*) polarization of the coronal light; (*B*) changes of intensity of the corona with

the sunspot activity; and (*C*) the infra-red excess in the corona.

(*A*) *The polarization.* The effort to measure the polarization of the solar corona has a very long history dating back to observations in 1851 by Edlund. Considerable effort was made during the 1930's and 1940's to measure the polarization. Prior to these measurements the suggestion was made by Schwarzschild that the Sun's corona was an electron gas and that the light from the corona came from scattering by free electrons. It should therefore have radial polarization in the sense of the magnetic vector in the electromagnetic wave. Because of the attractiveness of this suggestion, most of the polarization measurements assumed radial polarization and consisted of measurements of the intensity with two analysers at 90° to one another. Such a measurement cannot establish the direction of the polarization.

In only four cases, to our knowledge, has the measurement of the polarization been made with three or more analyser angles in order to determine the direction of the polarization. Van de Hulst[4] in his review article ends his discussion with the remark, "Every time that this method has been used, [to measure the direction of polarization] it has suggested deviations from the normal direction. Theoretically, such deviation seems entirely impossible".

Note added in proof: In addition to the measurements quoted, von Klüber[4a] has observed radial polarization in the eclipse of 1952 (sunspot minimum).

One of our purposes here is to review some of the measurements of the direction of polarization and to consider whether such deviations are actually impossible. The probable source of non-radial polarization would be synchrotron radiation from electrons in magnetic fields. The energies required for the electrons are enormously high—higher than one is used to considering. Conversely, if it proves possible to demonstrate the existence of synchrotron radiation, then previous ideas of the solar corona must be considerably revised.

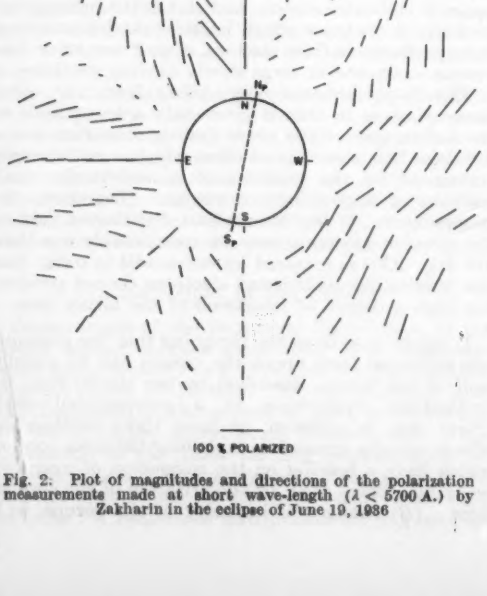

Fig. 2. Plot of magnitudes and directions of the polarization measurements made at short wave-length ($\lambda < 5700$ A.) by Zakharin in the eclipse of June 19, 1936

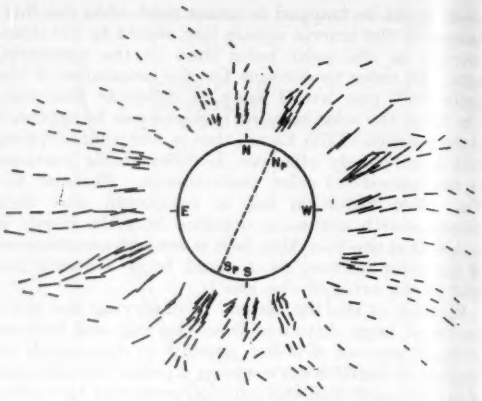

Fig. 3. Plot of the polarization magnitudes and directions obtained by Vashchakidse in the eclipse of September 21, 1941

The first measurement of the direction of the polarization was made by K. G. Zakharin during the eclipse of June 19, 1936[6]. His results are summarized in a table giving magnitudes and directions of polarization in two colours of visible light. These measurements have been plotted in Fig. 2, in which are shown the direction and magnitude of polarization as a function of position around the Sun. If these measurements are, in fact, correct, two remarkable effects are evident. One is the very great deviation from radial polarization, in some directions almost 90°; and the other is the almost complete absence of polarization in other directions. Both these facts could be understood if, in addition to radial polarization due to Thomson-scattering by the trapped particles, there is synchrotron radiation polarized in some other direction. For example, when zero polarization occurred one might assume that the synchrotron radiation gave polarized light perpendicular to the radius vector in approximately equal intensity to that due to Thomson-scattering with a radial polarization. Where the polarization is at almost 90° to the radius vector, one must assume that the overwhelming source of polarized light would be synchrotron radiation. It should be pointed out that high intensity of polarized light due to synchrotron radiation in the visible region requires electrons with kinetic energies of one to ten thousand million electron volts. However, the observations seem to indicate that such polarizations do exist.

The second measurement of polarization with three directions of the analyser was made by Richtmyer during the 1939 eclipse, but unfortunately Prof. Richtmyer died before he could study his plates. These plates are reproduced in various places[7] and have been studied by M. Schmitt[8]. Schmitt measured the polarization along several streamers which were evident in Richtmyer's pictures and found that in the case of one of these streamers the direction of polarization deviated from radial by 30°.

The third measurement of polarization to which we refer was made by Vashchakidse[9] during the eclipse of 1941. The result of plotting these measurements is shown in Fig. 3. It appears here that although there is a lack of radial polarization, the

extent of this deviation is not nearly as great as that observed by Zakharin. It should be pointed out, however, that in this eclipse the total light emitted from the corona was only half that observed in the 1939 eclipse, and there may have been fewer particles injected which were capable of producing synchrotron radiation.

The fourth measurement, that of K. Saito, is not available to us.

We believe that very careful measurements of the direction of polarization, especially in the infra-red, would be a crucial test of the presence of synchrotron radiation in the corona. In the infra-red the intensity of the Thomson-scattered light should be reduced and the intensity of the synchrotron radiation increased.

(*B*) *Changes of the corona with the sunspot cycle.* It is well known that the shape and intensity of the solar corona are greatly dependent on the sunspot cycle. At sunspot maximum the corona seems to be almost spherical and approximately twice as bright as the sunspot minimum corona, which is extended in the equatorial plane. Such changes, although difficult to understand in the gravitation equilibrium model, could certainly be explained qualitatively by the injection and acceleration at sunspot maximum of larger numbers of particles in the trapping field. It will be interesting to see whether fluctuations of the solar cycle occur in the Van Allen radiation around the Earth to the same extent that they appear to occur in the solar corona.

(*C*) *Infra-red excess.* A number of authors[10] have measured the ratio of total radiation to visible for the corona and the Sun itself. These measurements are generally in agreement that the coronal emission is rich in infra-red, relative to the Sun.

The explanation of the coronal light as due to Thomson-scattering of sunlight is unable to account for this phenomenon. On the other hand, the presence of synchrotron radiation almost certainly implies that at longer wave-lengths the corona should become brighter. It is worth noting that the infra-red excess in the 1936 eclipse in which Zakharin's polarization measurements were obtained was approximately 4 to 1 relative to the Sun[11]. In other words, the corona had relatively four times more non-visible light than did the solar spectrum. In the corona of the 1941 eclipse, in which smaller deviations from radial polarization were observed, the infra-red excess was also reduced[12].

The strengths of the solar magnetic fields required by our model are determined by the properties of the synchrotron radiation. Unfortunately, the determination of B depends not only on the intensity of synchrotron radiation but also on the energy spectrum of the trapped particles.

If one knew the spectral properties as well as the intensity of the synchrotron radiation one could determine both the energy spectrum of the trapped particles and the strength of the trapping fields. We can, however, make some estimates assuming the following:

(1) The synchrotron radiation is equal in intensity to the Thomson-scattered light in the visible region. This leads to equation 1:

$$B^2\gamma^2F = 50 \qquad (1)$$

(2) The critical frequency, or frequency of maximum emission, of the synchrotron radiation corresponds to the wave-length of red light. This condition is represented by equation 2:

$$\gamma^2B = 3\times10^7 \qquad (2)$$

In our equations B is magnetic field in gauss; γ is the ratio of electron energy to rest mass energy; and F is the fraction of the electrons which have energy γ mc.^{2}. We have also computed T, the lifetime for energy loss by radiation, and R, the ratio of the energy density of the radiating electrons to $B^2/8\pi$. This latter quantity should not exceed 1. The equations for T and R are:

$$T_{(\text{sec.})} = \frac{5\times10^8}{\gamma B^2} \qquad (3)$$

$$R = 1{,}000\,\frac{F}{B^2}\,\gamma \qquad (4)$$

In Table 1 are the results of these calculations for γ's of 10^2 to 10^4. A γ of 10^2 is ruled out because it leads to an unreasonable value of B; a γ of 10^4 is ruled out because the energy density is too high for containment in the field. Values of γ near 10^3 lead to reasonable estimates of F, B and R.

Table 1

γ	F	B	T	R
10^2	5×10^{-10}	3,000 gauss	0·5 sec.	5×10^{-12}
10^3	5×10^{-8}	30 gauss	500 sec.	5×10^{-6}
4×10^3	10^{-6}	2 gauss	3×10^4 sec.	0·5
10^4	5×10^{-6}	0·3 gauss	5×10^5 sec.	500

In order to check the hypothesis suggested above, precise measurements of the polarization over a wide spectral range, as well as of the spectral composition of the light, will be important. We are now planning to carry out such measurements during the eclipse of October 2, 1959. In addition to studying the polarization and quality of the spectrum, we hope to be able to determine whether the solar corona has an intensity maximum close to the Sun similar to that observed at larger distances in the Van Allen radiation. Our measurements will be made with a television-type scanning scheme using photomultipliers. The problem of reversal in photographic emulsions has previously masked the possible presence of a maximum. It would seem reasonable that if such a maximum exists it would have to occur close to the Sun, since the injection into the solar trapping field is from the inside, whereas in the case of the Van Allen radiation around the Earth the injection probably takes place from the outside. The basic similarity of the two phenomena, however, suggests, perhaps, that the Van Allen radiation belt could be called the corona of the Earth.

We wish to thank Prof. J. R. Winckler for stimulating and helpful discussions.

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NEWS and VIEWS

Electrical Engineering at Saskatchewan: Prof. W. E. Lovell

Prof. W. Edward Lovell, who will retire as head of the Department of Electrical Engineering in the University of Saskatchewan, Saskatoon, on June 30, 1960, graduated at the University of Manitoba. He joined the staff of the University of Saskatchewan as a lecturer in 1923, and was appointed professor and head of the Department of Electrical Engineering in 1924. Prof. Lovell has been closely concerned with problems of electric power generation and distribution in the prairie provinces of Canada. He has been widely acclaimed as an outstanding teacher in the field of electric power production, and he has been responsible for introducing many innovations in the undergraduate laboratories.

Prof. Lovell was president of the Association of Professional Engineers of Saskatchewan in 1937, and has been chairman of the Saskatoon Community Planning Commission since 1940. He was a member of the Street Railway Commission in 1932.

Prof. N. F. Moody

Mr. N. F. Moody, head of the Basic Circuit Research Division of the Canadian Defence Research Board, Ottawa, has been appointed professor of electrical engineering in the University of Saskatchewan, Saskatoon, from July 1, 1959.

During the Second World War, he was at the Telecommunications Research Establishment, Malvern, and, in collaboration with Prof. F. C. Williams, he worked on various aspects of radar research, including automatic range gates, guidance of aircraft by means of radar techniques, and aircraft blind-landing systems. Mr. Moody was head of the Electronics Branch of the National Research Council Atomic Energy Project at Chalk River, Canada, during 1947–51. In 1951 he was appointed superintendent of electronics in the High Explosive Research Establishment of the Ministry of Supply, Fort Halstead, and he was responsible for the elaborate telemetering equipment used in the tests of the first British atomic bomb. Mr. Moody returned to Canada in 1953 and became the first head of the Basic Circuit Research Division at the Defence Research Board, Ottawa.

During the past six years he has established one of the leading laboratories in Canada for the study of semi-conductor devices, and his personal contributions in the field of ultra-high-speed transistor circuits have been notable. Prof. Moody will be associated at Saskatoon with Prof. Arthur Porter, who left the Imperial College of Science and Technology in 1958 to become dean of engineering at the University of Saskatchewan (see *Nature*, 181, 1178; 1958).

Space Research in Britain

In replying to further questions in the House of Commons on April 27 regarding space research, the Minister of Supply, Mr. A. Jones, declined to add to his previous statement (see *Nature*, May 2, p. 1231) and asked members to await the promised statement of policy. Nevertheless, the Parliamentary Secretary to the Ministry of Supply, Mr. W. J. Taylor, welcomed an adjournment debate on this subject on April 29 and outlined briefly what Britain had already done in this field, assuring Mr. de Freitas, who opened the debate, that the Government is giving active consideration to the question of Britain engaging in an Earth satellite programme. British activity in this field began to take shape in the autumn of 1953, when the Royal Society considered the value and practicability of research into the upper atmosphere, extending roughly to about 150 miles from the Earth's surface. In April 1955 five university groups started experimental work and they have since been joined by a sixth. The Royal Aircraft Establishment undertook to develop rocket vehicles suitable for these experiments and, by agreement with the Australian Department of Supply, arrangements were made for the rockets to be launched at Woomera. The first trial flight of the rocket developed for this purpose, *Skylark*, took place early in 1957, and a series of twelve fully instrumented firings began in the spring of 1958.

The Editors regret that, due to difficulties in the printing industry, some sections of this issue of "Nature" have had to be curtailed

Mr. Taylor stressed the magnitude of Britain's effort in tracking the Earth satellites launched by the Americans and the Russians and in analysing the data received from them, referring to the international recognition of the very fine work of the Department of Scientific and Industrial Research stations at Slough and Singapore, the Jodrell Bank radio-telescope and of some Ministry of Supply establishments. New information had been obtained about the shape of the Earth and a contribution made to the study of air-drag on satellites and its interpretation in terms of air density. While, however, the Government is considering the possible use of the *Black Knight* research vehicle and the *Blue Streak* ballistic vehicle in an Earth satellite programme, it could not allow any such use to interfere with the place *Blue Streak* took in the military programme of deterrence. It had sought the considered views of the best scientific opinion in Britain, and Mr. Taylor repeated that an announcement as to whether and in what way the United Kingdom should extend its research activities into outer space would be made very shortly. Replying to specific points raised by Mr. de Freitas, Mr. Taylor thought that Mr. de Freitas's estimate of £10 million to £20 million as the cost of a satellite research programme over the next five years might be too modest; further design studies would be needed before reliable estimates could be made. He welcomed, however, the suggestion that the satellites should be designed as laboratories in space, working in a definite scientific field, but he did not think that a satellite programme was so crucially important that the scientific well-being of the country stood or fell by it. He also thought that the dangers of scientific emigration should not be exaggerated, nor the effect

on it of any decision regarding space research. There was, and would continue to be, great scope for international co-operation outside the Commonwealth in this field, and Mr. Taylor referred particularly to the International Committee on Space Research, one of the fruits of the International Geophysical Year. It was also expected that an *ad hoc* committee of the United Nations could meet shortly to consider what international action regarding co-operation in the peaceful uses of outer space could profitably be taken at inter-government level.

Manchester Literary and Philosophical Society: New Building

THE foundation stone of the new building of the Manchester Literary and Philosophical Society was laid at 36 George Street—the site of the old building destroyed in an air raid in December 1940—by the Lord Mayor of Manchester, Alderman J. E. Fitzsimmons, on April 30, and it is expected that the new meeting house will be ready in about a year's time. In welcoming the Lord Mayor, the president of the Society, Mr. C. E. Young, said that the building destroyed in 1940 had housed the Society for at least 140 years. Before the building was acquired in 1799 the Society, which was founded in 1781, had met first in the Assembly Coffee House, which was probably in the Exchange Buildings of that day, and later in a room at the back of Cross Street Chapel. Mr. Young referred to the association with the Society of such famous scientists as Percival, Dalton, Joule, Schunck and Roscoe, among its past presidents, and afterwards, in a speech at an informal luncheon, expressed the hope that the Society would become a meeting place of minds from town, university and industry, representing and serving the everyday intellectual life of the city.

Thanking the Lord Mayor on behalf of the Society, Lord James expressed the conviction that there is even more important work for such a Society to do to-day. When it was founded and for the next half-century, an educated man could take all knowledge for his province. That is no longer possible, and one of our greatest needs is to encourage communication between men educated in different fields. Lord James thought the Literary and Philosophical Society could make a very important contribution to the establishment of channels of communication between specialists. By providing a formal and informal meeting place for people with widely differing interests, the Society was preparing to maintain and extend its original task of stimulating the intellectual life of the city. That task would call for the support and encouragement, financial and otherwise, of a considerable body of sympathizers. Mr. Young referred particularly to the need for adequate financial support for operation and maintenance, although the membership of the Society is now 320, the highest in its history. Virtually all the contents of the old centre were destroyed by fire, including the records, the library and Dalton's manuscripts and other relics, and there is a deficit of some £10,000 on the £35,000 which the new building will cost, to be met before the Society can be re-established on a sound financial footing. It is hoped and expected that the lecture rooms will be used by many of the numerous scientific and other societies which are active in Manchester. Both Mr. Young and Lord James paid tribute to the work of the Society's honorary treasurer, Mr. H. Hayhurst, in promoting the scheme.

The Vienna Declaration and Nuclear Disarmament

THE Vienna Declaration, adopted by the participants in the Third Pugwash Conference held at Kitzbuhel, Austria, on September 14–19 and at Vienna on September 20, 1958 (cf. *Nature*, 180, 358; 1957), has been sent, with a questionnaire and a letter signed by Lord Russell, to scientists all over the world to ascertain whether there is general support for the ideas expressed in the Declaration and for further activities based on it. The Declaration affirms that the only restraint against the use of nuclear weapons in war would be agreements not to use them, which were concluded in time of peace: the possession of such weapons is relatively unimportant, as in any future major war each belligerent would feel compelled to undertake immediate production of nuclear weapons, and for a major industrial power less than one year should suffice. The statement recognizes that any agreement on disarmament, and particularly on nuclear disarmament, requires a measure of control to protect every party from possible evasion, and because of the technical difficulties, the need for policies encouraging international trust and reducing mutual apprehension is stressed. The signatories believe that, as scientists, they have an important contribution to make towards establishing trust and co-operation among the nations, and that science can best serve man if it is free from interference by any dogma imposed from outside, and can exercise its right to question all postulates, including its own. The scientific tradition of mutual understanding and of international co-operation can, and should be, extended to many fields of technology.

Quality of Protein Feeding-Stuffs

SIR SOLLY ZUCKERMAN, a member of the Agricultural Research Council and chairman of its Protein Quality Group, writes: The Agricultural Research Council has organized a collaborative investigation into the measurement of the quality of protein feeding-stuffs. Several centres are taking part in a trial designed to establish whether any rapid laboratory tests can give a reliable indication of the feeding value of such materials. Specimens of these materials, some of which have been imported, were kindly supplied by manufacturers who have included, in response to a request to cover as wide a span of quality as possible, samples known to be bad, as well as samples within the commercial range. When it has been completed, the results of the whole trial will be published by the Committees as a whole. In the meantime, however, it is probable that members of the staff of the participating laboratories will be publishing in various journals papers describing work in which some of these samples have been used, but which deal with matters not directly related to the correlations which the joint effort is designed to elucidate. It has been agreed that in any such papers the samples used will be described by the code numbers uniformly used by the whole group.

Australian and New Zealand Association for the Advancement of Science

A REPORT on the future of the Australian and New Zealand Association for the Advancement of Science published in the *Australian Journal of Science* for October 21, 1958, advocates discontinuing the Handbook and Report and making greater use of the *Australian Journal of Science*, which it is proposed to publish monthly. Conferences every year and a half are recommended instead of annually or biennially,

but no change in the names or number of the Sections are recommended apart from the formation of a new Section corresponding with the Assembly of Corresponding Societies in the British Association. Stress is laid on keeping the public informed on what science is doing as the primary function of the Association and the report recommends that as much sectional time as possible should be devoted to meetings of the symposium type which present more general matters from several angles.

Journal of Medicinal and Pharmaceutical Chemistry

This new journal (1, No. 1, February 1959. Pp. 120. Published bi-monthly; subscription price 115*s*. or 15 dollars per annual volume including postage. New York: Interscience Publishers, Inc.; London: Interscience Publishers, Ltd., 1959) will be mainly of interest to the synthetic chemist engaged in the manufacture of drugs and to pharmaceutical chemists and pharmacologists whose line of research lies in the drug-action relationship. "Chemical-biological correlative work" is defined by the editors as one of the main fields which the new journal wishes to cover. Papers are to be accepted in English, French or German. The increasing number of drugs produced by the chemical industry, all of which need biological evaluation, probably created a need for a special journal to deal with the quantity of work done; the future will show whether this need could not have been covered by the existing journals.

Commonwealth Bursaries Scheme

The fifth annual report on the Royal Society and Nuffield Foundation Commonwealth Bursaries Scheme covers the year ended December 31, 1958 (pp. 8. London: Royal Society and Nuffield Foundation, 1959), in which 26 bursaries, totalling £13,974, were offered, compared with 18, totalling £8,423, in 1957. Of these 8 were for visits from the United Kingdom, 12 for visits to the United Kingdom and 3 for visits between countries of the Commonwealth other than the United Kingdom; three offers were not accepted. Of the 67 applications considered during the year, 44 were for visits to the United Kingdom, 11 for visits from the United Kingdom and 12 for visits between Commonwealth countries other than the United Kingdom. Most of the applications were of a high standard. Besides the Nuffield Foundation's grant of £5,000, which was received for 5 years from 1959, £2,000 was received through a Parliamentary Grant-in-aid to the Royal Society, £1,200 from Canada, £800 from Australia, £400 from South Africa, £322 10*s*. from Pakistan, £200 from New Zealand, and £100 each from Ceylon, Rhodesia and Nyasaland.

Studies in Indonesia

The Jajasan Siswa Lokantara, the local Indonesian foundation for granting fellowships to foreigners studying in the country, is ready to consider suitably qualified applicants in any subject of study. This foundation has considerable funds at its disposal and is independent of government departments; it is prepared to guarantee all reasonable local expenses, including living costs and fares within Indonesia, but owing to the considerable variations in cost of living throughout the territory has refrained from fixing a definite monthly allowance. The intention is to operate a flexible and reasonably generous allowance scheme which will enable Fellows to study where they wish (so far as internal conditions and transport facilities allow), and to establish close relations with Indonesians working in similar fields. Further information can be obtained from the Universities Section, Education Division, British Council, 65 Davies Street, London, W.1.

First All-India Congress of Zoology

The first All-India Congress of Zoology, sponsored by the Zoological Society of India, which was to have been held last year but was postponed, will be held at Jabalpur during October 24–29, at the invitation of the University of Jabalpur. Further information can be obtained from the General Secretary, Dr. B. S. Chauhan, c/o Zoological Survey of India, 34 Chittaranjan Avenue, Calcutta 12.

Announcements

Dr. A. A. Moss has been appointed deputy keeper of the Department of Mineralogy in the British Museum (Natural History).

Prof. John Read, professor of chemistry in the University of St. Andrews, has been selected for the 1959 Dexter Award which is administered by the Division of History of Chemistry of the American Chemical Society.

Dr. H. Albert Barker, professor of microbial biochemistry in the University of California, Berkeley, has been awarded the Neuberg Medal for outstanding work in biochemistry and, in particular, for his discovery of the coenzyme corresponding to vitamin B_{12}. The Neuberg Medal was created to honour Carl Neuberg, a pioneer in biochemistry. It is given annually by the American Society of European Chemists for outstanding contributions in the field of biochemistry.

An International Congress on Radiology will be held in Munich during July 23–30. Further information can be obtained from the Congress-Sekretariat, Reitmorstrasse 29, Munich 22.

A symposium on Fluorine Chemistry is being arranged under the auspices of the Chemical Society, to be held in the University of Birmingham during July 15–17, covering topics in organic, inorganic and physical chemistry. Further details can be obtained from the Secretary, Fluorine Symposium, Chemistry Department, The University, Edgbaston, Birmingham 15.

The twenty-first International Congress of Physiological Sciences (Physiology and Pharmacology) will be held in Buenos Aires during August 9–15 under the presidency of Prof. Bernardo A. Houssay. Further information can be obtained from the Secretary, Twenty-first International Congress of Physiological Sciences, Facultad de Ciencias Médicas, Paraguay 2151, Buenos Aires, Argentina.

The Gordon Research Conferences for 1959 will be held during June 15–September 4 at Colby Junior College, New London, New Hampshire; New Hampton School, New Hampton, New Hampshire; and Kimball Union Academy, Meriden, New Hampshire. The conferences were established to stimulate research in universities, research foundations and industrial laboratories. This purpose is achieved by an informal type of meeting consisting of scheduled lectures and discussion groups. Further information can be obtained from W. George Parks, Director, Department of Chemistry, University of Rhode Island, Kingston, Rhode Island; after June 15 from Colby Junior College, New London, New Hampshire.

INLAND WATERWAYS IN BRITAIN

THE debate in the House of Lords on February 17 on a motion of Lord Ammon's directing attention to the report of the Bowes Committee of Inquiry into Inland Waterways preceded by a few days the publication of the Government proposals in a White Paper (Cmnd. 676). The debate, nevertheless, was welcomed by the Earl of Gosford, who, in replying on the debate for the Government, said that the various suggestions made will receive careful consideration. Lord Gosford pointed out that the main points in the Committee's report were that capital development should continue to be concentrated on the 300 or 400 miles of the more than 2,000 miles of inland waterways in Great Britain which carry busy and remunerative traffic. Another 900 miles carry some commercial traffic but are unremunerative, and the Committee recommends the rehabilitation to navigation of these waterways with the development of other uses and revenue. The remaining waterways have outlived their commercial utility and the Committee proposes a case-by-case review to assess their possibilities and prepare schemes for future treatment.

Lord Gosford indicated that the Government was fully aware of the many interests concerned in these proposals, including water undertakings, drainage and river boards, farmers, anglers, naturalists and amenity, but his reply foreshadowed the Government's proposals in the White Paper. They cover an interim policy over about two years in which some of the Committee's ideas can be tried out experimentally, but while endorsing the Committee's objectives in redevelopment and proposing to set up an Inland Waterways Redevelopment Advisory Committee, and accepting that legislation to facilitate redevelopment is needed, the Government is not prepared at present to commit itself to the detailed procedural machinery suggested. It also considers that the cost of redevelopment should be borne by those who benefit from it.

The Nature Conservancy in its last annual report expressed the view that the findings and recommendations of the Bowes Committee providing for the setting up of a waterways redevelopment board to prepare schemes for redevelopment, but envisaging the participation as new owners of river boards or drainage authorities, water undertakings or local authorities, and, exceptionally, special trusts, are regarded as meeting adequately the Conservancy's needs. Lord Gosford, however, did not refer to the boards, although this question was specifically raised by Lord Latham in the debate. In the White Paper the Government accepts the view that proposals for costly schemes of improvement are not justified in present circumstances and that instead there should be a gradual extension of the work which has already been carried out. The Committee recommended that the Great Yarmouth Port and Haven Commissioners should be reconstituted on a wider basis of membership and with greater powers, and the Nature Conservancy appears to accept this view. Nevertheless, and although the Committee also urged that the demand now for the use and development of the rivers and Broads was greater than ever and their deterioration could be stayed and improvement ensured by a revision in the powers of the Commissioners and a small increase in their income, the Government is not prepared to introduce the recommended legislation without first thoroughly exploring the administrative proposals between the various interests concerned. Discussions between representatives, however, have already begun, including those of the National Parks Commission, although it may be noted that the Committee is not impressed by the arguments for making the Broads a national park.

THE NATIONAL OCEANOGRAPHIC COUNCIL

REPORT FOR 1957–58

THE annual report of the National Oceanographic Council for 1957–58* reviews progress in the two main fields of work of the National Institute of Oceanography: marine physics and marine biology. With a staff of eighty-four (not counting officers and crew of R.R.S. *Discovery II*) and with an annual expenditure of £201,000 mainly met by United Kingdom and Commonwealth Government grants, the effort has been chiefly devoted to studies of the interaction between atmosphere and ocean, water movement on large and small scales, biological fertility of the oceans, and the distribution of marine animals. Basic knowledge of physical and biological processes has also been applied to practical problems.

R.R.S. *Discovery II* spent two periods of nearly three months measuring currents in the Gulf Stream and working stations across the Atlantic in latitudes 48°, 32° and 24° N., as part of the Institute's contribution to the International Geophysical Year. A large part of the fuel was kindly provided by the Woods Hole Oceanographic Institution, and United States oceanographers and vessels collaborated in this international attack on the problems of ocean circulation. Deep-water isohalines, computed from the electrical conductivity of samples taken at a range of depths at stations about 8 miles apart, revealed greater irregularities in water properties and motion than had been inferred from earlier, less-precise observations.

The technique of tracking deep currents by means of neutrally buoyant floats, pioneered at the Institute, has stimulated similar measurements elsewhere, as well as theoretical studies of ocean circulation. Floats have now been made which will send out signals for a few hours each day for a month. The more practical needs of the fisherman and the fisheries scientist for a knowledge of sub-surface currents has also been

* Annual Report of the National Oceanographic Council, 1 April, 1957–31 March, 1958. Pp. v+39. (Cambridge: At the University Press, 1958.) 5s. net.

met by improvements in a simple but ingenious method of recording the orientation and inclination of a container moored in the current. Echo soundings made along 12,000 miles of the ship's track have included detailed surveys of the Porcupine Bank (from which rocks were collected) and large sand waves on the continental shelf to the west of the English Channel which attain 40 ft. in height and 3,000 ft. between crests.

Research on processes operating at the sea surface, in particular wind waves, has been mainly directed at finding a convenient way of measuring the directional distribution of wave energy and to the development of an adequate physical theory relating the wave spectrum to wind, stability, and other factors governing turbulent shear and the exchange of energy between atmosphere and ocean. Theoretical studies of the curvature of random surfaces and the distribution of intervals between 'crests' of random functions enable directional information to be derived from aerial photographs of waves. Other theoretical studies of far-reaching practical significance have been concerned with the estimation of the mean wave energy from the highest waves in a record; with the accuracy of wave measurements using accelerometers in buoys or ships; with the vertical components of motion of a ship heading into sea waves; with the relative merits of empirical formulæ for wave and swell prediction; and with long waves and storm surges. Tidal records initiated by the Institute at Stornoway and Lerwick support the theoretical conclusion that surges arriving from the Atlantic are amplified on passing down the east coast and suggest that large surges are most likely when intense storm depressions travel from the south-west for a long distance along the continental shelf.

The Institute is still the chief protagonist of the wave-interference theory of microseism generation. While there is still need for clarification, it has proved possible to track isolated storms in the North Atlantic by analysis of microseism activity at Kew. There is a practical application in tracking storms and giving warning of heavy swell on coasts facing oceans that have little meteorological cover. An electronic seismograph has been constructed which allows exceptional magnification in the 3–10-sec. range. Other instruments in the course of development include an airborne radar sea-swell recorder and a pressure recorder of high precision which, when used to record waves, can be calibrated by tidal observations.

The Institute's biological work falls into three broad groups: the distribution and taxonomy of marine animals, particularly oceanic plankton, based to a large extent on the *Discovery* collections; the bionomics of whales; and the productivity of oceanic waters. Objects of study last year included: the alcyonarians (revision of the pennatularian genus *Umbellula*); certain families of the isopods (fifteen species, of which five and one family are new); deep-living chaetognaths (including a new species with a method of fertilization hitherto unknown in this group); the distribution and life-cycles of the Antarctic copepod *Calanoides acutus* and of the euphausid *Euphausia triacantha*.

During a spring cruise to the Canary Islands, additions were made to the collection of photographs of oceanic animals, ranging from small planktonic forms to moderately large cephalopods, including a dramatic sequence at 500 m. in which a squid was attacked and eaten by another squid. Observations on the composition and numbers of phytoplankton and zooplankton, compared with those made in the same waters on previous cruises, emphasized the remarkable stability and constancy of the crops in this part of the open ocean in different years. During this and other cruises, particular attention has been paid to the factors controlling rates of assimilation of carbon in regions of dense and sparse phytoplankton, and to the significance of the ratios of chlorophyll *a* and *c* in coastal and oceanic regions.

Largely through the help of whaling inspectors and companies the Institute has been able to examine ear plugs and ovaries from several hundred whales. Ear plug laminations and the corpora albicantia in the ovaries are indices of age and provide valuable evidence on life-histories, rates of ovulation, and on year-to-year changes in the stock. Recovery of whale marks has consolidated and extended earlier inferences about distribution, migrations, and interchange between the south-west Atlantic and south-east Pacific in both directions.

Finally, it is encouraging to record that governmental aid has been increased to allow for expansion of staff, that four investigators from overseas have played a considerable part in the work, and that the effort devoted to the problem of fish detection is welcome evidence of increasing collaboration between the physicists and the biologists.

C. H. Mortimer

THE ROCKEFELLER FOUNDATION

REPORT FOR 1957

THE annual report of the Rockefeller Foundation for 1957, in which appropriations totalled 42,798,916 dollars, including 8,298,260 dollars for medical education and public health, 5,255,235 dollars for biological and medical research, 5,621,400 dollars for the agricultural programme, 3,570,525 dollars for the social sciences and 5 million dollars to the American University of Beirut, stresses the broadening basis of co-operation among governments, with which the work of the Foundation has brought it into close contact at the local level in every continent. For half a century the Rockefeller boards have been intimately involved in the international community of science and scholarship, particularly in the financial support of scholarship and science abroad, largely through the universities, but using various techniques. Special emphasis is laid on the value of the Foundation's fellowship programme, under which 7,432 men and women from 98 countries, including 1,306 from the United States, have held fellowships for advanced study in 80 countries: of these, 248 received awards in 1957, when 532 persons from 53 countries pursued their training or research in 104 institutions in 20 countries.

Discussing the characteristics of this international community of science and scholarship, the report emphasizes that it is open to all who wish to contribute ideas and its respect is bestowed on the basis

of merit, not of authority. Sweeping beyond national frontiers, the community is linked by a strong network of communication through books, scientific and learned periodicals, meetings, and a persuasive flow of travel and personal correspondence, in which much has been done to remove the barrier of language. Development now involves the fullest participation in this community of science and scholarship and it is suggested accordingly that present tensions underline the need to seek out and cherish those elements of modern life which bind men together across national and cultural frontiers. In the United States alone 66,000 persons participated in educational exchange during the year, 52,000 from 145 countries or territories coming to study or teach, and 14,000 Americans visiting 50 countries for similar tasks. Secondly, governments have demonstrated their ability to set political issues aside at selected points to co-operate effectively for the welfare of their peoples, and if international politics has not yet found practicable means for settling disputes, the field of politics could be reduced by steadily broadening the basis of co-operation on the long-range humane interests of man.

The President's review selects for emphasis the Polish science programme on which in May 1957, following a visit of Foundation representatives to Poland, grants of 300,000 dollars for purchase of scientific materials and supplies, books and periodicals and of 175,000 dollars for fellowships for younger Polish scientists to visit other countries were made. The new grants for Poland are concentrated on medical education, public health, the biological and medical sciences and agriculture, as the Ford Foundation, also at the invitation of the Polish Government, has initiated a programme in social sciences and humanities. Of the 25 fellowships awarded, 19 have already commenced study, most in the United States, but 4 in the United Kingdom, and grants-in-aid were also made to 21 more mature Polish scientists.

In referring again to the public health aspects of nuclear energy, the report suggests that the complicated and difficult problems which will accompany massive use of nuclear energy involve special responsibilities for the universities; the Foundation is accordingly supporting several broad programmes of special advanced training. That at the New York University Institute of Industrial Medicine stresses the chemical and radiobiological toxicological problems of the nuclear energy industry, particularly quantitative aspects of environmental contamination; at Harvard and at Pittsburgh problems of air-borne chemical and radiobiological contamination are stressed, while at Johns Hopkins University the primary emphasis is in the expanded Department of Biophysics: each of these institutions is receiving 500,000 dollars.

Dealing with the Foundation's operating programmes, the President reviews in some detail its world-wide study of the virus infections transmitted to men and domestic animals by the bite of arthropods. Under it more than 70 distinct viruses have been discovered, 37 of which have been shown to be members of one or other of three immunologically related families, 27 being located in South America, 24 in the Ethiopian region and 15 in the Oriental region, including India, Malaya, the Indonesian Archipelago, Indo-China and South China. He refers to the rapidity with which, in 1957, following the discovery in India of an agent belonging to the Russian tick-borne complex of viruses, the etiology of the disease and its major epidemiological features were elucidated. This shows the utility of the Rockefeller Foundation's virus programme. A special appropriation of 100,000 dollars will assist the Indian Government with its vaccination campaign and enable Indian scientists to study vaccine production techniques in the United States.

Under the agricultural programme, now in its seventeenth year, the Mexican, Columbian, Chilean and Indian agricultural programmes, the last differing from the others in including direct connexion with a graduate school of agriculture and the correlation of the food crop research projects with the research and training activities of the school, will receive support. The Mexican and Columbian programmes have participated in the National Research Council project for preserving indigenous varieties of corn by systematically collecting samples of every extant variety in the High Andean region, Central America and Mexico, while all three Latin American units are co-operating in the international wheat rust nursery of the United States Department of Agriculture.

THE PLANT BREEDING STATION, NJORO, KENYA

By H. C. THORPE
Department of Agriculture, Kenya

THE new research laboratories of the Plant Breeding Station, Njoro, were opened on August 29, 1958, by the Speaker of the Kenya Legislative Council, Sir Ferdinand Cavendish-Bentinck. He was deputizing for the Minister of Agriculture, the Hon. Michael Blundell, who was unable to be present owing to illness. The ceremony was held in the presence of representatives of agricultural and milling interests, and of a large gathering of farmers from all over the country. After the opening, visitors were shown the field work in progress and conducted through the new laboratories where exhibits had been arranged demonstrating the various problems under investigation.

The building was designed and built by the Ministry of Works, in consultation with the Department of Agriculture, to cater for the special needs of the plant breeding team. The east wing houses the threshing room and cereal-selection laboratory, with space for the storage of sheaf material and packeted seed; special features are the provision of adequate well-lit benching for grain selection, and the arrangement of the layout for ease and efficiency of handling the breeding material. The west wing houses the pathology section, which deals with cereal diseases with special reference to wheat stem rust. The main body of the building consists of the administrative offices, a cereal chemistry laboratory, library, dark

room, stores and service rooms, as well as private rooms for individual research officers. Much thought has gone into the planning of the layout, and the architect has designed a light and airy building of pleasing character, yet retaining its strictly functional purpose, for a total cost of £15,000.

The Station is situated at 7,100 ft. in the Kenya Highlands, virtually on the equator, in one of the main wheat-growing areas of the country. It is the headquarters of the Plant Breeding Section, and the main programme on wheat, barley and oats is concentrated there. The present staff of the section, including two plant breeders working on maize at subsidiary stations, comprises eight graduate officers and additional supporting staff.

History records that the early settlers in Kenya started growing wheat at the beginning of the century; but that they suffered great loss due to rust attacks. Although a certain amount of desultory work was carried out from 1910 onwards, it was not until 1928 that wheat-breeding was put on an organized basis as a result of the visit of the late Sir Rowland Biffen, and the advice he gave to the Government to set up a wheat-breeding station. In that year the original station was built at Njoro and the late Capt. J. G. L. Burton, one of Biffen's pupils, was appointed to the first post of plant breeder. He was assisted by Mr. R. J. Lathbury, who later took charge when Burton retired. The Station has, until recently, remained virtually unchanged since those early days.

The present period of expansion dates from 1952–53 when development funds were first made available. The provision of improved facilities had become especially necessary due to the severe outbreaks of rust causing great damage to existing wheats and the urgent need to produce resistant varieties. The development has entailed recruitment of extra staff, purchase of additional land and specialized equipment, and now the construction of the new laboratories. It is especially satisfactory that farmers themselves, by agreeing to a cess on wheat, have made a substantial contribution to this development. Certain further projects, however, connected with rust pathology and maize breeding await the grant of special funds.

The plant breeding team is fortunate in being provided with such excellent new laboratories and modern facilities. Indeed, these are essential to the necessary intensification of the research programme to provide the rust-resistant wheats Kenya so urgently needs.

THE GALACTIC SYSTEM AS A SPIRAL NEBULA

THE view that the Galaxy might have a spiral structure has been expressed almost since the first discoveries of such structure in nebulæ. It was not until 1951, however, that part of the galactic spiral structure was actually found. Work in this field has recently been summarized in a report on the progress of astronomy by J. H. Oort, F. T. Kerr and G. Westerhout (*Mon. Not. Roy. Astro. Soc.*, **118**, 379; 1958). The first definite discovery of spiral structure was accomplished by W. W. Morgan, S. Sharpless and D. E. Osterbrock, using the luminosity criteria for *O* and *B* stars developed by Morgan. They determined the distances of groups of these stars, and they showed that the regions of ionized hydrogen excited by the stars were arranged in long chains which formed parts of spiral arms. By a remarkable coincidence, radiation on 21-cm. wave-length from interstellar hydrogen was detected at about the same time as the visual work was done, and by 1954 a comprehensive picture of the spiral structure of the Galaxy could be assembled from radio observations. The importance of hydrogen-line radiation at 21 cm. arises partly because of the transparency of interstellar dust to radio waves and partly because the study of this radio spectrum line gives discrimination in distance. Actual distances can only be inferred from radial velocities, and this involves the assumptions of circular motion and of equality of motions of stars and gas in any particular region of the Galaxy.

In order to transform radial velocities into distances, we must know how the circular velocity in the Galaxy varies with the distance from the galactic centre, and we must know the distance of the Sun from the centre. If we know the distance and circular velocity of the Sun we can derive, from the radio observations themselves, the variation of circular velocity with distance from the centre for distances smaller than that of the Sun. The distance from the Sun to the centre has been obtained directly from the study of the distribution of cluster-type cepheid variables concentrated near the centre. The best estimate obtained in this way is 8·2 kiloparsecs, the accuracy being limited by the uncertainty in the interstellar absorption and in the absolute magnitude of the variables. The circular velocity, and an independent estimate of the solar distance, can be obtained by combining radio observations with the constants of differential galactic rotation obtained from the proper motions and radial velocities of distant stars. The best available estimate of the circular velocity of the Sun is 215 km./sec. For regions of the Galaxy farther from the centre than the Sun we cannot determine the rotational velocities from radio observations, and we must rely on values computed from models of the Galaxy based on stellar densities. The rotation curve derived by M. Schmidt from all available data shows a maximum of about 225 km./sec. at a distance of 7 kiloparsecs from the centre.

From profiles of the 21-cm. line one can now derive the density distribution of the neutral hydrogen in the Galaxy. Most of the hydrogen is confined to a flat disk, with a half-density whole-width of 220 parsecs. This hydrogen disk is remarkably flat; nowhere within 8 kiloparsecs from the centre do the points of maximum hydrogen density deviate by more than 75 parsecs from the galactic plane, although there are rather larger deviations in the outermost parts of the Galaxy. In contrast to this arrangement about the galactic plane the distribution in the plane shows great irregularities. The hydrogen outlines the spiral arms of the Galaxy. The Sun is situated near the inner edge of one arm; the latter probably contains the Orion association, and the whole arm is now called the Orion arm. Outside this, at a distance from the galactic centre of between 10 and 12 kilo-

parsecs, there is a conspicuous arm, called the Perseus arm, which contains the double cluster in Perseus. Inside the Orion arm, between 6 and 7 kiloparsecs from the centre, there is the Sagittarius arm. The hydrogen disk appears to terminate at about 15 kiloparsecs from the centre. The maximum density of hydrogen in the spiral arms is about 2 atoms/cm.^{3}; between the arms the density may fall to less than 0·05 atoms/cm.^{3}. These results are all compatible with the classification of the Galaxy as a spiral nebula of Hubble's type *Sb*. It is estimated that about 2 per cent of the total mass of the Galaxy exists in the form of interstellar gas. In the central region there is an arm at a distance from the centre of 3 kiloparsecs. The arm passes between the Sun and the centre, and in addition to participating in galactic rotation, this arm appears to be expanding with a velocity of 53 km./sec. away from the centre. This arm can be seen in absorption against the source Sagittarius *A* situated at the galactic centre. The existence of this expanding arm (and of other smaller features which are expanding) renders the rotation curve quite uncertain near the galactic centre.

The astounding flatness of the galactic interstellar medium is of importance in connexion with the internal motions in the medium. It provides a measure of the average random motion of the interstellar clouds, and it indicates that there must have been a considerable exchange of momentum from the inner to the outer parts of the disk. A fundamental question is whether spiral structure is a problem of gas dynamics (and magnetic fields) or of stellar dynamics. There seems no doubt that the spiral structure of galaxies is closely related to their content of interstellar gas, and the latter may be a prerequisite for spiral structures to develop. Supergiants and other young stars are situated in the spiral arms; there is as yet little evidence relating to the distribution of older stars and gas. The Galaxy rotates in a period of about 200 million years, and the effects of differential galactic rotation would severely distort any existing arms. The arms must either be smoothed out and replaced within this time or be maintained by some unknown mechanism independently of galactic rotation. There are reasons for believing that interstellar magnetic fields must be involved in such a mechanism. Interstellar polarization of the light of distant stars indicates the presence of magnetic fields, in many cases along the spiral arms. Part of the continuous radio emission from the Galaxy is closely concentrated in the galactic plane, and must be connected with spiral structure; the radiation appears to be of non-thermal origin, and may originate in synchrotron radiation from electrons moving in interstellar magnetic fields. The existence of radio coronae of the Galaxy and of the Andromeda nebula also supports the idea of the importance of magnetic fields. Nevertheless, we are forced to the conclusion that we have made little progress towards understanding the origin of spiral structure. We have, however, a much-improved knowledge of what has to be explained.

R. H. Garstang

IODINE AND COPPER UPTAKE BY SOIL ORGANIC MATTER

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ALTHOUGH the soil organo-metallic complex concept of Bremner *et al.*[1] has been confirmed by the work of Beckwith[2], these workers do not specify the nature of the organic component of the complex. However, the relationship found between the replacement capacity of soil organic matter and its phenolic hydroxyl group content[3] points to material derived from lignin containing these groups being the organic component concerned. These considerations have led to an investigation of the relationships between the phenolic hydroxyl group content and the organo-metallic complex-forming capacity of the organic matter of a number of limed and unlimed soils from the Kenya Highlands. Wide ranges of *p*H values and organic matter contents were covered.

The phenolic hydroxyl group contents of the organic fractions of the soils were measured by the iodine uptakes according to the hypoiodite method of Norman and Peevy[4] as modified by Moodie[5]. The capacities of the same organic fractions to form organo-metallic complexes were measured by their copper uptakes by Lees's method[6] slightly modified.

Copper was taken as the complexing metal as it is known to have a greater ability to compete for sorption sites on organic matter than most of the other bivalent metals present in soil[3,7]. Preliminary investigations of the copper uptakes have indicated the possibility that added copper is preferentially absorbed by the organic fraction of a soil, and it is intended to offer evidence of this at a later date. Table 1 gives the copper and iodine uptake values obtained for the twelve soils studied.

The results show that increasing amounts of copper are taken up by the soil organic fractions with increasing *p*H values, the relationship between the two variables being curvilinear. No correlation

Table 1. Iodine and Copper Uptake by Soil Organic Matter

Soil	Total carbon (per cent)	Lime applied	*p*H	Copper uptake (m.equiv./gm. C)	Iodine uptake (m.equiv./gm. C)
*K*27	2·76	No lime	4·74	0·215	5·19
*K*20	2·86	4,400 lb./acre in 1953	5·16	0·268	4·55
*K*5	2·86	8,150 lb./acre in 1953	5·45	0·304	4·72
*K*48	2·53	17,740 lb./acre in 1953	6·12	0·735	4·35
*M*6	4·29	No lime	4·96	0·221	4·97
*M*8	4·22	15,000 lb./acre in 1954	6·01	0·628	4·76
MF	4·31	No lime	5·63	0·418	5·55
*MG*38	3·58	No lime	5·92	0·708	5·63
*MG*36	5·62	No lime	5·74	0·386	4·86
*ML*19	5·39	No lime	5·38	0·290	5·35
*ML*16	4·93	No lime	5·47	0·382	5·49
*E*127	2·44	No lime	5·45	0·409	5·62

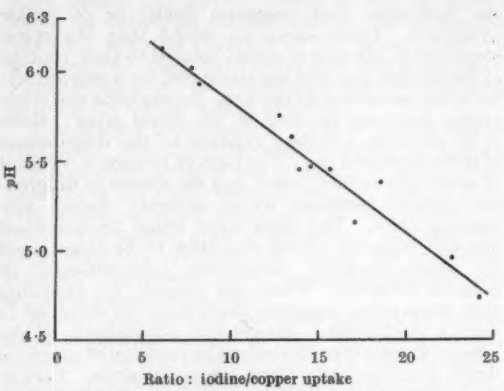

Fig. 1. Relation between *p*H and the ratio iodine/copper uptake by organic matter of some Kenya soils

is found between *p*H and iodine uptake. However, if the ratio iodine/copper uptake is plotted against *p*H then a rectilinear relationship results (Fig. 1), the regression equation of which is given by:

$$R = 90{\cdot}58 - 13{\cdot}85\ p\text{H}\ (r = 0{\cdot}977)$$

A possible explanation of these results is suggested as follows.

It has long been thought probable that when the soil organic matter is treated with alkali, the compounds of high molecular weight undergo breakdown into smaller units, and it does not seem unreasonable to expect that at low concentrations of alkali the extent of this breakdown will depend upon the strength of alkali used.

If this effect can be extended to the action of lime in the field it might be expected that breakdown will occur with liming and the size of the organic structures existing in soil organic matter will be controlled by the *p*H of the soil. It has been shown that for the twelve Kenya soils studied, covering a fairly wide range of *p*H and total carbon values, a rectilinear relationship exists between *p*H and the ratio iodine/copper uptake. This relationship is taken as reflecting a close connexion between soil *p*H and the molecular complexity of its organic matter. The copper uptake, being determined at the *p*H of the soil, would be stoichiometrically proportional to the molecular compounds existing at that *p*H, and the iodine uptake, which is determined in the presence of 3 *N* sodium hydroxide solution, would be stoichiometrically proportional to the components of the compound. Therefore, the ratio of iodine/copper uptake would be a measure of the complexity of the molecular compound existing at that particular soil *p*H.

Evidence of the relationship between the copper- and the iodine-absorbing fractions of soil organic matter and the increase in copper absorption as compounds of high molecular weight are broken down into their constituent units when treated with alkali was obtained by treating four of the soils with concentrated ammonia. Excess ammonia was removed by drying followed by leaching with 0·5 *M* sodium sulphate solution. The copper uptake of the treated soil combined with the amount of copper required to precipitate the copper-precipitable constituents of the leachate was found to be much greater than the copper uptake of the untreated soil and to approximate to its iodine uptake (Table 2).

Table 2. Copper Uptake of Ammonia-treated Soil Organic Matter

Soil	Copper uptake untreated soil (m.equiv. Cu/gm. C)	Copper uptake of ammonia-treated soil (m.equiv. Cu/gm. C)	Iodine uptake (m.equiv. I/gm. C)
*K*5	0·304	4·72	4·72
*M*8	0·628	3·97	4·76
*ML*16	0·382	3·31	5·49
*K*48	0·735	5·06	4·35

The narrowing of the iodine/copper ratio with the increase of *p*H resulting from liming would appear to reflect a breaking down of soil organic complexes with parallel increase in mobility which may be of great importance in accounting for a number of the observed effects of liming.

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STRUCTURE OF ANHYDROUS SODIUM SOAPS AT HIGH TEMPERATURES

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THE present work is part of a systematic investigation of the liquid-crystalline structure of association colloids[1].

It is well known that anhydrous sodium soaps exhibit five liquid-crystalline allotropic phases at temperatures in the range 100–320° C.; the transition points have been determined by different techniques[2], in particular, light transmission measurements, microscopic examinations under polarized light and dilatometric observations. The names of the phases and the transition temperatures are given in Table 1, taken from the paper by M. J. Vold, M. Macomber and R. D. Vold[3].

H. Nordsieck, F. B. Rosevear and R. H. Ferguson[4] have studied the different phases of sodium palmitate

Table 1

Name	Sodium laurate (C_{12})	Sodium myristate (C_{14})	Sodium palmitate (C_{16})	Sodium stearate (C_{18})
Subwaxy	100° C.	107° C.	117° C.	117° C.
Waxy	141	141	135	132
Superwaxy	182	176	172	167
Subneat	220	217	208	198
Neat	255	245	253	257
Isotropic melt	336	310	295	288

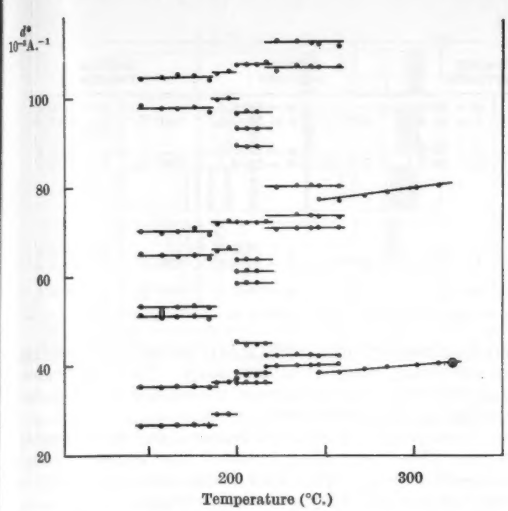

Fig. 1. Reciprocal spacings as a function of temperature for sodium laurate

by X-ray diffraction techniques. They recognized two main regions in the X-ray diagrams: the central part ($0{\cdot}01$ A.$^{-1} < s < 0{\cdot}10$ A.$^{-1}$; where $s = 2\sin\theta/\lambda$), where they observed several sharp lines, and a region in which s is of the order of $0{\cdot}25$ A.$^{-1}$, where they found one or two diffuse bands. They considered the position and the profile of the bands as evidence that the soap molecules are packed in a hexagonal array and rotate freely around their axes. These authors did not try to determine the crystal lattices responsible for the sharp lines.

An infra-red absorption study[6] has shown that in all the high-temperature phases, from the subwaxy to the isotropic melt, the paraffin chains are disordered and that their configuration is similar to that of a paraffin liquid.

The purpose of our investigation was to determine the liquid-crystalline structure of the different phases of sodium laurate, myristate, palmitate and stearate, by the interpretation of the sharp lines occurring in the central region of the X-ray diagrams.

We have used a Guinier-type focusing camera, which was evacuated and provided with a bent quartz monochromator. The samples were held in an electric furnace and the temperature was maintained with an accuracy of 0·5 deg. C. The soaps were produced by neutralizing alcoholic solutions of the fatty acids (supplied by the Eastman Kodak Co.). Water and alcohol were eliminated by heating the sample for 1–2 hr. *in vacuo* at 120° C. in the cell in which it was enclosed for the X-ray investigations. These precautions for avoiding water and alcohol seem to be sufficient, since X-ray diagrams show no appreciable dependence upon moisture.

We have taken at least one hundred X-ray diagrams of each soap in the temperature-range 100–350° C. In the central part of the diagrams we have observed a large number of sharp diffraction lines (sometimes more than twenty). These sharp lines form a typical pattern for every phase; the transition temperatures are in good agreement with those previously determined (see Table 1). We have plotted in Fig. 1 some of the experimental results for sodium laurate.

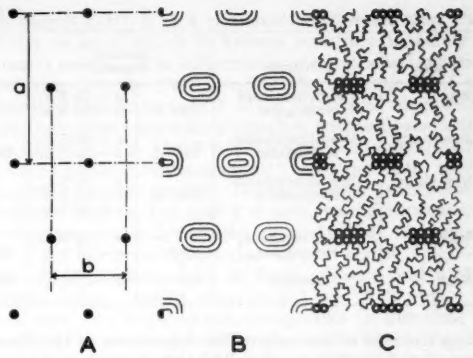

Fig. 2. (*A*) Two-dimensional centred rectangular lattice. (*B*) Electron-density projection along [001]. Contours are at equal arbitrary intervals, with the zero contour omitted. (*C*) Schematic structure of the rectangular phases

At s of the order of $0{\cdot}2$–$0{\cdot}3$ A.$^{-1}$, the diagrams show the diffuse bands described by Ferguson and coworkers[4]; but we have observed that at low temperatures (more particularly in the subwaxy and the waxy phases) the bands have a fine structure, which is the more detailed the lower the temperature.

Since the experimental results are essentially the same for the four soaps investigated, we propose to describe separately the structure of every phase.

Subwaxy, waxy, superwaxy, subneat. The observed spacings are consistent with a two-dimensional centred rectangular lattice (space group *c2mm*; ref. 5) (see Fig. 2*A*). By way of example, we compare in Table 2 the observed spacings (sodium laurate at 166° C.) and the calculated spacings for a rectangular cell with sides $a = 75{\cdot}2$ A., $b = 30{\cdot}9$ A.

Table 2

Indices $hk0$		200	110	310	400	020	220	130	330
s (in 10^{-3} A.$^{-1}$)	exp.	26·2	35·2	51·0	53·1	64·3	70·0	97·7	105·2
	calc.	26·6	35·0	51·4	53·2	64·7	69·9	97·9	105·0

The structure (see Fig. 2 *C*) consists of a set of parallel ribbons, indefinite in length, packed in the rectangular array. The polar groups of the molecules fit into the ribbons. The paraffin chains, in a liquid-like state,

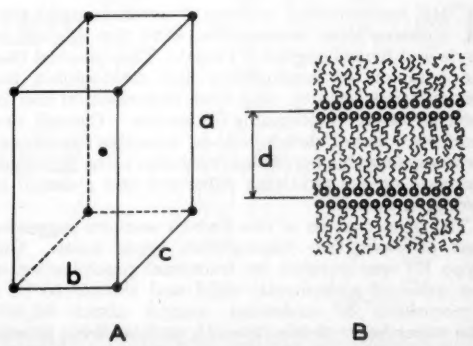

Fig. 3. (*A*) Bravais unit cell for the three-dimensional body-centred orthorhombic structure. (*B*) Schematic structure of neat soap

Table 3

Phase	Structure	Sodium laurate		Sodium myristate		Sodium palmitate		Sodium stearate	
Waxy	two-dimensional rectangular	166° C.	$a = 75{\cdot}2$ A. $b = 30{\cdot}9$ A.	155° C.	$a = 80{\cdot}0$ A. $b = 34{\cdot}5$ A.	152° C.	$a = 86{\cdot}6$ A. $b = 38{\cdot}3$ A.	140° C.	$a = 80{\cdot}0$ A. $b = 40{\cdot}3$ A.
Superwaxy	two-dimensional rectangular	195° C.	$a = 68{\cdot}5$ A. $b = 30{\cdot}3$ A.	193° C.	$a = 69{\cdot}0$ A. $b = 33{\cdot}6$ A.	195° C.	$a = 74{\cdot}2$ A. $b = 36{\cdot}0$ A.	198° C.	$a = 72{\cdot}5$ A. $b = 37{\cdot}9$ A.
New phase	three-dimensional orthorhombic	205° C.	$a = 55{\cdot}5$ A. $b = 28{\cdot}3$ A. $c = 32{\cdot}7$ A.						
Subneat	two-dimensional rectangular	234° C.	$a = 49{\cdot}8$ A. $b = 27{\cdot}0$ A.	233° C.	$a = 53{\cdot}8$ A. $b = 29{\cdot}1$ A.	236° C.	$a = 56{\cdot}2$ A. $b = 30{\cdot}6$ A.	238° C.	$a = 62{\cdot}4$ A. $b = 34{\cdot}4$ A.
Neat	lamellar	290° C.	$d = 25{\cdot}3$ A.	271° C.	$d = 27{\cdot}5$ A.	278° C.	$d = 29{\cdot}1$ A.	285° C.	$d = 30{\cdot}6$ A.

fill up the rest of the cell. The intensities of the lines are in good agreement with this model; this is confirmed by the electron-density distribution calculated with the observed amplitudes and the signs corresponding to the model (Fig. 2 *B*).

The dimensions of the unit cell depend on the soap and on the phase; they are given in Table 3. For every soap, the higher the temperature the more symmetrical (closer to a hexagonal cell) is the shape of the unit cell.

Neat. In this phase the X-ray diagrams contain three or four lines giving Bragg spacings in the ratio 1 : 1/2 : 1/3 : 1/4, characteristic of a lamellar structure (one-dimensional lattice). The structure of the neat phase is sketched in Fig. 3 *B*.

The neat is the only phase in which the spacings are strongly dependent on temperature. The coefficient of linear thermal expansion is negative and its value approximately -5×10^{-4}/deg. C. (note that, at room temperature, the thermal expansion coefficient of the large parameter of crystalline soap is $+5 \times 10^{-5}$/deg. C.). We have observed the same thermal effect in the liquid-crystalline structures of soap–water systems[7] and we have interpreted it to mean that hydrocarbon chains are liquid-like in character.

Isotropic melt. X-ray photographs taken at a temperature a few degrees above the melting-point contain two or three fairly sharp bands corresponding to the Bragg spacings of neat soap. This observation suggests that the structure of the melt is quite similar to that of the neat phase.

A new phase. In sodium laurate, we have observed a new phase occurring between the superwaxy and subneat phases. This new phase does not seem to have been noticed by previous workers.

The lattice is three-dimensional body-centred orthorhombic, space group *Immm* (see Fig. 3*A*); the dimensions are: $a = 55{\cdot}5$ A., $b = 28{\cdot}3$ A., $c = 32{\cdot}7$ A. The spacings and intensities of the diffraction lines are in good agreement with a model in which the polar groups of the soap molecules pack into slightly asymmetrical disks, which pile up in the crystal, and the paraffin chains fill up the rest of the cell. This phase is not found in the other soaps.

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GENETIC CONTROL OF HUMAN HAPTOGLOBIN SYNTHESIS

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THE haptoglobins, a group of serum proteins that combine with hæmoglobin, were first studied by Jayle and his colleagues in Paris[1]. They showed that the complex of hæmoglobin and haptoglobin has peroxidative activity, and that this reaction can be used to assay haptoglobin in serum. Connell and Smithies[2] have developed a sensitive technique for analysing this peroxidase activity, using hydrogen peroxide as the oxidizing substrate and guaiacol as hydrogen donor.

The observations of the French workers suggested that at least two haptoglobin types exist. One (type II) was purified by fractional precipitation of the urine of a nephrotic child and shown[3] to be a glycoprotein of molecular weight about 85,000. The other haptoglobin (type I), purified from human serum, was reported[4] to have a similar composition and a molecular weight of about 160,000. It was concluded that type I might be a dimer of type II.

With the introduction of the starch gel technique of zone electrophoresis, Smithies[5] recognized three types of human sera, which were later[6] termed types 1–1, 2–1 and 2–2. It became clear that the proteins differing in the several serum types were haptoglobins[6], Jayle's type II haptoglobin corresponding to Smithies's 1–1, and Jayle's type I to Smithies's 2–2, respectively[7].

Smithies and Walker[6] offered a simple hypothesis for the inheritance of the haptoglobin types. They suggested that a single pair of allelic genes, Hp^1 and Hp^2, segregate in human populations, the three genotypes Hp^1/Hp^1, Hp^2/Hp^1 and Hp^2/Hp^2 corresponding to the phenotypes 1–1, 2–1 and 2–2, respectively. Other family studies[8] have confirmed that this is the usual mode of inheritance of haptoglobins. However, atypical segregation has been observed[9] and modified phenotypes reported[8,10]. Moreover, in some populations relatively high fre-

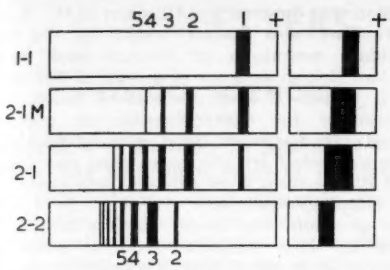

Fig. 1. Relative mobilities of haptoglobins and their complexes with hæmoglobin from the several phenotypes at *p*H 8·4, left, in starch gels, right, in starch blocks. The numbers show the supposed degree of polymerization of components and + marks the anodal side

quencies of individuals without demonstrable haptoglobins have been found[10,11]. These variations will be discussed below.

Bearn and Franklin[12] have recently studied haptoglobins and their complexes in the ultracentrifuge. In sera of type 1–1, haptoglobin was reported to have a sedimentation coefficient of 3–4 *S*., increasing to 6 *S*. on addition of hæmoglobin, in agreement with the observations of Guinand *et al.*[3]. In the other serum types, haptoglobins and their complexes with hæmoglobin showed several peaks in the ultracentrifuge, most of those from type 2–2 sera having higher sedimentation coefficients than those from type 2–1 sera. In view of this complexity, and the numerous haptoglobin components demonstrated by starch gel electrophoresis of 2–1 and 2–2 sera[3,9,13], it was concluded that the genetical control of haptoglobin synthesis is "probably exceedingly complex". Moreover, since one of the components in the ultracentrifuge was present only in 2–1 sera, Bearn and Franklin suggested that the haptoglobin genes might interact in the heterozygote to produce a hybrid product like those described in doves[14] and rabbit[15] and human[16] blood groups.

This system is of unusual interest, then, in relation to the genetic control of protein synthesis, for not only does the heterozygous individual produce proteins different from those in either homozygous type, but also in two of the types the haptoglobins consist of several fractions. No other genetic system is known in which a single locus controls the specificity of more than one protein[17]. It therefore seems worth while to present a hypothesis for the genetic control of haptoglobin formation which brings this group of proteins into line with others and appears to provide an adequate explanation for all the findings so far reported.

The differences between haptoglobins in the three types will first be briefly recapitulated (Fig. 1). In type 1–1 sera only a single haptoglobin is present, having a relatively high mobility on filter paper and starch block electrophoresis and also in starch gels at *p*H 8·4–8·6[3,5,12,13]. Ultracentrifugal studies show only a single component of relatively low molecular weight[3,12]. Type 2–2 haptoglobins appear as a single component when submitted to electrophoresis on filter paper and in starch blocks, having a lower anodal mobility than type 1–1 haptoglobin. However, when 2–2 haptoglobins are submitted to electrophoresis in starch gels, numerous components, becoming fainter towards the origin, are resolved. Since these components do not differ in charge (as shown by the uniform mobility on filter paper and in starch gels), it is a reasonable inference that they differ in size, which is known to affect mobility in starch gels[18]. This inference is strengthened by their heterogeneity in the ultracentrifuge. Indeed, if the frictional coefficients of 1·4–1·6 calculated for purified haptoglobins[3,4] are taken together with sedimentation coefficients of 2–1 and 2–2 haptoglobins extrapolated to zero protein concentration[12], it seems likely that a polymer series is present. The lowest (4 *S*.) component, present only in 1–1 and 2–1 sera, would represent a monomer (molecular weight about 80,000), the second (6–7 *S*.) a dimer (molecular weight about 160,000), the third a trimer, and so forth, dimers and higher orders being present only in 2–1 and 2–2 sera. In 2–1 sera one haptoglobin component is identical in mobility in starch gel electrophoresis with the haptoglobin of type 1–1. Other haptoglobin bands consistently show a higher electrophoretic mobility in starch gels than the haptoglobins in type 2–2 sera. In modified type 2–1 sera (2–1 *M*), the first component is greater and the second component is smaller than usual, and very little of the other components is present; this type is especially common in Africans[10].

An attempt will now be made to provide an explanation for these seemingly confusing observations. It is suggested that recently synthesized haptoglobin molecules (of molecular weight about 80,000) have complementary combining sites (1 and 2) through which aggregation can take place under favourable conditions. The protein product of the Hp^1 gene is Hp1, which has only a single combining site (Fig. 2). Since the complementary combining site is lacking, only monomers are present in Hp^1/Hp^1 homozygotes. The product of the Hp^2 gene, which can be termed Hp2, has both complementary combining sites 1 and 2, and also a lower negative charge at *p*H 8·4 than Hp1. Hence in Hp^2/Hp^2 homozygotes the haptoglobin molecules can combine with one another, so that polymers are formed. It would appear that a small proportion of dimers, a high proportion of trimers and progressively lower proportions of tetramers and higher polymers result. The different polymers are separated by starch gel electrophoresis, so that numerous bands are seen, becoming progressively fainter towards the origin.

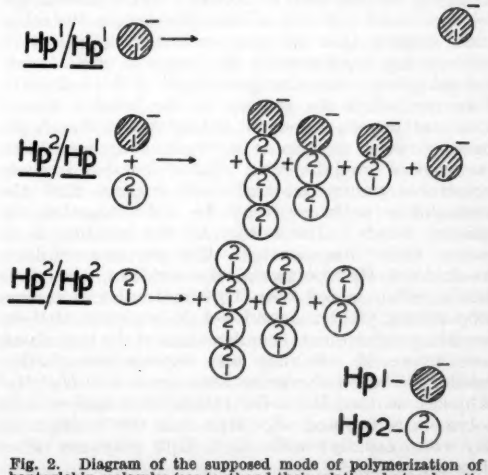

Fig. 2. Diagram of the supposed mode of polymerization of haptoglobin molecules (see text) and their relative electrophoretic mobilities in starch gels. Only the first three polymers are shown. Origin to the left and anode to the right. For convenience the polymers are shown as linear; in practice they are more likely to be globular

In heterozygotes, Hp1 and Hp2 molecules are formed side by side (like A and S hæmoglobins in sickle-cell heterozygotes). These form mixed polymers, each containing only one Hp1 molecule, which has a single combining site. The commonest polymer is the dimer, with progressively fewer trimers, tetramers and higher orders. Since Hp1 has a greater negative charge than Hp2 at *p*H 8·4, the mixed Hp1, Hp2 polymers would have a higher electrophoretic mobility than the polymers Hp2, Hp2 on both starch block and starch gel electrophoresis. However, in the absence of masking of charged groups, all Hp1, Hp2 polymers would have the same charge and therefore a similar electrophoretic mobility in starch blocks or on filter paper ; and the same would be true of Hp2, Hp2 polymers. Nevertheless, if approximately equal proportions of Hp1 and Hp2 are formed in heterozygotes, all the Hp2 but only a fraction of Hp1 molecules will polymerize ; hence the residual Hp1 molecules will form a component identical with Hp1 in Hp^1/Hp^1 homozygotes.

The genetic variant 2–1 *M* is readily explained by supposing that a higher proportion of Hp1 than usual is synthesized. There will therefore be more residual Hp1 monomer and less Hp1, Hp2 dimer than in type 2–1, with only very small proportions of higher polymers.

The genetic control of haptoglobin synthesis is analogous in some respects to that of the human hæmoglobin types. Hæmoglobins S and C differ from normal adult hæmoglobin (A) in charge : this appears to be due to the substitution of a valine or lysine residue for glutamic acid, respectively, in each half-molecule[19]. Although the differences between them are so small, S is the only hæmoglobin which polymerizes in the absence of oxygen into highly asymmetrical units, this process being responsible for sickling and birefringence of erythrocytes in susceptible subjects[20]. Moreover, hæmoglobin S can form mixed polymers with hæmoglobins C or A [20], in somewhat the same way that it is suggested that Hp2 molecules can form mixed polymers with Hp1. Here the analogy ends, for hæmoglobin polymers seem to be due to the formation of secondary intermolecular bonds which are easily disrupted, for example, by oxygen or urea[20]. The starch gel electrophoretic patterns of haptoglobins, on the other hand, suggest that the polymerization is relatively stable, being unaffected by the presence of 8 *M* urea and relatively wide changes of *p*H (1·7 to 9·0)[2,13]. Moreover, when the protein in one band is eluted from starch gels and again submitted to starch gel electrophoresis, it runs as a single component with its original mobility[2,13]. These results exclude metastable polymerization[21] and suggest that the haptoglobin polymers may be held together by primary bonds. The nature of the bonding is at present under investigation. If it proves possible to break down Hp2 polymers, the resulting monomers should, when mixed with Hp1 molecules in various proportions, yield 2–1 and 2–1 *M* patterns, thereby providing experimental confirmation of the hypothesis here advanced. It may also explain two further problems : why polymerization is limited in Hp^2/Hp^2 subjects, so that low-order rather than higher-order polymers occur, and why Hp1 does not interact (at any rate, rapidly) with Hp2, Hp2 polymers when they are mixed[12,13].

The other genetical problem posed by the haptoglobins is the presence in several populations of persons without demonstrable haptoglobins, to which attention was directed by Allison *et al.*[11]. A number of sera have now been re-tested by the sensitive peroxidase technique of Connell and Smithies[2]. Only two British sera out of a total of 632 examined over a period of four years have failed to show haptoglobins by electrophoretic or peroxidative analysis. On the other hand, no haptoglobins could be demonstrated by the peroxidase reaction in 14 out of 20 sera from West Africans previously thought to be ahaptoglobinæmic.

The question then arises whether lack of haptoglobins in these subjects could be secondary to hæmolytic anæmia or liver disease[22,23]. This seems unlikely. The British subjects without haptoglobins on repeated tests were apparently healthy adults, with normal blood counts ; and similar observations on British subjects have been presented by Harris *et al.*[8]. The Africans studied were also apparently healthy adults, who should have had considerable resistance to malaria. Ahaptoglobinæmia in Africans was not correlated with the presence of abnormal hæmoglobins S or C, which in the heterozygous condition are not associated with low haptoglobin levels, although absence of haptoglobins has been reported in patients homozygous for abnormal hæmoglobin genes[22]. Furthermore, the recent demonstration by Giblett[10] that some American Negroes lack haptoglobins rules out the possibility that ahaptoglobinæmia is always secondary to malaria, malnutrition, liver disease or other factors in the African environment.

It seems likely, then, that a small proportion of Caucasians and a higher proportion of Africans congenitally lack haptoglobins. In some cases, at least, this does not seem to be due to homozygosity for a third, 'silent' allele at the *Hp* locus. Mothers of ahaptoglobinæmic subjects who have been of phenotype 2–1 could not have been carriers of the silent alleles[6,9,12], on the hypothesis here advanced or any other plausible hypothesis. The most likely genetic explanation for ahaptoglobinæmia seems to be homozygosity for a recessive suppressor gene at another locus[6,9,23], but further family studies would be desirable.

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LETTERS TO THE EDITORS

PHYSICAL SCIENCES

Calculation of Erosion in Space from the Cosmic-Ray Exposure Ages of Meteorites

VERY little is known about erosion in space—the action of dust, gas, ions and electrons in wearing away material from exposed surfaces in interplanetary space. A cosmic-ray exposure age is obtained from the measurement of one radioactive and one stable spallation isotope produced in a meteorite by cosmic rays. A new interpretation of this age indicates that it sets an upper limit to the erosion-rate of a meteorite in space. The more obvious interpretations in terms of meteoritic break-up and planetary perturbational effects may remain valid.

The cosmic-ray exposure age is determined from the spallation isotope content of a meteorite, with the assumption that the spallation-rate does not vary with time. If erosion occurs in space, however, the spallation-rate in the distant past would have been smaller than in recent times because of the greater amount of shielding material. Hence, due to erosion in space, the measured cosmic-ray exposure age is less than the interval since the meteorite was exposed to cosmic rays. Consequently, the cosmic-ray exposure age gives an upper limit to the rate of erosion in space.

We shall base our discussion on the argon exposure age[1] of the Sikhote–Alin meteorite. The value of 5×10^8 years was obtained[1] from the number of argon-38 atoms (stable isotope) and the decay-rate of argon-39, a radioactive isotope with a half-life of 260 yr. The tritium (helium-3) ages of two irons[2] and one stone[3] meteorite have also been measured; however, the argon exposure age is subject to fewer uncertainties than the tritium (helium-3) age.

We shall assume that the rate of production of argon-38 near the centre of a large iron sphere of radius R obeys the attenuation rule:

$$d^{38}A/dt = C \exp(-R/72) \text{ for } R \gg 100 \text{ cm.}$$

where C is a constant determined by the spallation properties of the iron nucleus and the cosmic ray flux.

The Sikhote–Alin meteorite was quite large: the argon-39 radioactivity in the measured sample was about one-ninth the activity expected in a small unshielded body. If the rate of erosion in space is designated by E and if r is the radius of the meteorite just before it struck the atmosphere, then the rate of production of argon-38 at t years ago was $C \exp\{-(r + Et)/72\}$. The present rate of production is $C \exp(-r/72)$. The measured cosmic-ray exposure age, 5×10^8 yr., is given by the expression $\int_0^T (d^{38}A/dt)dt/C \exp(-r/72)$, where T is the time that the meteorite existed as a body exposed to cosmic rays. If the attenuation rule is substituted into this expression, then the following relation is obtained:

$$5 \times 10^8 \text{ years} = (1 - \exp(-Et/72))/(E/72)$$

where E is the rate of erosion.

Since the time T is greater than the measured exposure age, 5×10^8 years, the upper limit to the rate of erosion E is $1{\cdot}5 \times 10^{-7}$ cm./yr. for an iron meteorite. This rate is somewhat smaller than an estimate made by one of us (F. L. W.)[4].

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Observations of the Lithium Lines in the Twilight Airglow in the Northern Hemisphere

THE recently published discovery of the lithium resonance line in the twilight airglow by Delannoy and Weill[1] and further observations of the emission by Gadsden and Salmon[2] have led to a discussion as to the origin of the lithium atoms in the upper atmosphere. Barbier, Delannoy and Weill[3] have considered the possibility of marine and meteoric origins for the lithium and have favoured the latter hypothesis because the derived abundance ratio lithium/sodium agreed better with the ratio for the meteoric material. Donahue[4] has suggested that the lack of linearity in the relation between sodium brightness and abundance could make ambiguous any conclusions based on the lithium/sodium brightness ratio alone. A third possibility suggested by the U.S.A. International Geophysical Year Committee[5], and supported further by Barber[6], is that the lithium originates from 'hydrogen' bomb tests at high altitude; it is suggested, in particular, that the high-altitude Johnston Island test of August 1, 1958, was responsible for the lithium emissions reported by Gadsden and Salmon from Antarctica and New Zealand. In view of the uncertainty of present knowledge of the behaviour of the lithium emission, an observation from a station in the northern hemisphere should be of interest.

Twilight spectra covering the region 6000–7000 A. have been obtained at Saskatoon regularly since January 1959. An $f/0{\cdot}8$ auroral grating spectrograph[7] was used to photograph the spectrum on Kodak 103 *a–F* plates. During the exposures the spectrograph was guided according to an exposure programme which kept it pointing at the point in the Sun–zenith plane where the geometrical shadow height was 80 km. Following such a programme, a 30-min. exposure may be obtained during a single twilight with a zenith angle range of 0–83°.

A very weak lithium line was obtained in this way on a plate exposed on four occasions during clear twilight during the period January 10–21, 1959. The total exposure time was 115 min. Comparison of this spectrum with the spectrum of a calibrated source of low brightness led to a value of 30 rayleighs for the zenith brightness of the lithium emission for a height of 80 km. of the geometrical shadow.

An estimate of the abundance of lithium in the upper atmosphere can be made from the observed

brightness. First the brightness when the lithium layer is fully illuminated must be estimated. The correction factor required depends on the distribution of the lithium with height. A value of about 5 for this correction factor was obtained by assuming the lithium distribution to be the same as that for sodium and then applying the theory developed by Hunten[8] to calculate a theoretical relation between brightness and geometrical height of shadow. A value of 150 rayleighs is thus estimated for the maximum zenith brightness when the lithium layer is fully illuminated by the Sun. The rate coefficient for the resonance fluorescence process for lithium obtained from the standard equation[9] is 17 photons/sec. per illuminated atom. This leads to the conclusion that the lithium abundance over Saskatoon in January was about 9×10^6 atoms/cm.^{2}. The sodium abundances estimated for the same period of observation from data kindly provided by Dr. D. M. Hunten may have been as high as 20×10^9 atoms/cm.^{2}. Consequently, the lithium – sodium abundance ratio was about 45×10^{-5} compared with 200×10^{-5} for meteors and $2 \cdot 3 \times 10^{-5}$ for sea water. If ionization is taken into account[8], it is possible that the abundance ratio derived from our measurements should be somewhat higher.

This result for the lithium – sodium abundance ratio, neglecting ionization, falls between the values for sea water and meteors, and cannot be considered as decisive evidence against either hypothesis in view of the uncertainties of the measurement. Moreover, since the chemical and physical properties of lithium and sodium (and their compounds) are considerably different, it is by no means certain that the abundance ratio in the upper atmosphere should be close to that of the source. As to the possibility of the lithium arising from bomb tests, it is possible that the lithium detected above Saskatoon in January is a remnant of that introduced into the atmosphere last August. However, it is also possible that the brightness of the emission in the southern hemisphere in August and September is associated with a seasonal variation of the lithium abundance; this abundance might well show a maximum about 2½–3 months after midwinter, as is the case for sodium in the northern hemisphere[10]. This question can be determined only by further systematic measurements.

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Sub-surface Temperatures on the Moon

Recently, Fremlin[1] has suggested that craters on the Moon may be filled with dust to considerable depths and that extremely high temperatures may be attained in them. While the effects described by him must exist, it should be pointed out that the extreme values he predicts are obtained by using a value for the thermal conductivity of the dust which is probably much too low and combining with this a value of the heat flux which is probably much too high. These points will be discussed in order.

Information about the thermal conductivity near the surface is obtained from optical and radio micro-wave observations of the variation of the surface temperature during an eclipse or throughout a lunation. These yield a value of $(K\rho c)^{-1/2}$, where K, ρ and c are the thermal conductivity, density and specific heat of the material near the surface (assumed to be independent both of temperature and depth). The optical eclipse observations of Pettit and Nicholson[2] were first reduced by Epstein[3], who deduced a value of 120 for $(K\rho c)^{-1/2}$, the units being c.g.s., calorie and degrees centigrade; since this is much higher than that for solid rock (about 20) he attributed it to a covering of 'pumice' or volcanic ash. Later, using more refined analysis and discussing both the variation of surface temperature during an eclipse and throughout a lunation, Wesselink[4], Jaeger and Harper[5], Lettau[6] and Jaeger[7] showed that Epstein's value was too low and independently obtained values of $(K\rho c)^{-1/2}$ of the order of 1,000. Similar values were obtained by Piddington and Minnett[8] and Jaeger[9] from the radio micro-wave observations. Taking $\rho = 2$, $c = 0 \cdot 2$, this corresponds to $K = 2 \cdot 5 \times 10^{-6}$: this low value was attributed to dust from meteoritic or other sources.

The agreement with experiment of calculations based on the above assumptions is good but by no means perfect, and all writers have recognized that it could be improved if the conductivity increased with either depth or temperature. In the absence of knowledge of a likely law of the variation of conductivity with temperature, discussion has been concentrated on the variation with depth, and it has been shown[5,8,9] that a rather better fit is obtained with a model consisting of a few millimetres of dust with $(K\rho c)^{-1/2} = 1{,}000$ resting on a substratum with $(K\rho c)^{-1/2} = 120$. This substratum might be composed either of tuffaceous material of volcanic origin or of material composed of rock fragments of all sizes produced by erosion due to thermal stresses. Recently, Muncey[10] has obtained a better fit with the radio micro-wave observations with a model in which both K and c are proportional to the absolute temperature with a value of $(K\rho c)^{-1/2}$ of 200–300 at 300° K. This low value, however, is inconsistent with the optical eclipse observations, since it gives temperatures in the umbral phase which are about 60 deg. too high.

Taking $\rho = 2$, $c = 0 \cdot 2$, the discussion above leads to a conductivity of the order of $2 \cdot 5 \times 10^{-6}$ at the surface, increasing in a few millimetres to 2×10^{-4}. These figures may be compared with the value of $7 \times 10^{-7}\ h^{1/2}$, where h is the depth below the surface, used by Fremlin. If this value 7×10^{-7} at a depth of 1 cm. were used for the surface conductivity, it would give calculated values of temperatures in the umbral phase of a lunar eclipse which are 25 deg. K. too low.

Turning to the values for the lunar heat flux used by Fremlin, he assumes that the average rate of heat production over the volume of the Moon due to radioactivity is that of 2 p.p.m. of uranium, corresponding to a total heat production of about 10^{20} cal./year. If the whole of this heat is lost by radiation from the surface, the surface heat flux would be $8{\cdot}4 \times 10^{-6}$ cal./cm.^{2} sec., which is seven times the terrestrial value. There is good evidence (Birch[11]) that the average rate of production of heat in the Earth is nearly the same as that of the chondritic meteorites, the total rate of production of heat in the Earth being of the order of $2{\cdot}3 \times 10^{20}$ cal./year, or not much more than twice that assumed by Fremlin for the Moon; if the whole of this heat were radiated from the surface, the surface heat flux would be $1{\cdot}4 \times 10^{-6}$ cal./cm.^{2} sec., which is in good agreement with the observed value of $1{\cdot}2 \times 10^{-6}$. An average heat production in the Moon at the chondritic rate would give a lunar surface heat flux of the order of $2{\cdot}3 \times 10^{-7}$ cal./cm.^{2} sec., and this is the value which probably should be assumed in the absence of evidence to the contrary. If the production of heat at the rate assumed by Fremlin were distributed over the volume of the Moon, melting would occur in a relatively short time: if, by a process of differentiation, it were concentrated into a crustal layer, not only would the concentration of radioactive elements near the surface be higher than in any terrestrial rocks, but, again, melting temperatures would be reached at shallow depths even in the absence of any blanketing dust. The triaxial figure of the Moon requires that there should have been substantial strength in the interior for a considerable time in the past.

Whether the blanketing effect described by Fremlin, which undoubtedly must exist, is of importance or not depends entirely on the values assumed for the conductivity and the lunar surface heat flux. Neglecting time effects in blanketing, lateral flow to better conducting regions, and the fact that the Moon is not in thermal equilibrium, all of which tend to reduce the magnitude of the effect, Fremlin deduces an excess temperature of 1,200° C. at a depth of 25 m.; with the same high heat flux and the conductivity of 2×10^{-4} deduced for the substratum from the eclipse observations, this figure would be reduced to 105° C. For a heat flux of the terrestrial value, $1{\cdot}2 \times 10^{-6}$ cal./cm.^{2} sec., the corresponding results would be 170° C. and 15° C.; while with the flux of $2{\cdot}3 \times 10^{-7}$ cal./cm.^{2} sec. corresponding to the chondrite model they would be 33° C. and 3° C., respectively.

Finally, it should be said that this matter is just within the range of experiment. Since the optical effects arise from a thin layer near the surface while the radio waves originate from a region some metres in thickness, the mean temperature as shown by micro-waves should be greater than that derived from optical observations if there is any general increase of temperature with depth due to the lunar heat flux. Using Fremlin's value $24\sqrt{h}$ for the temperature at depth h and a value $\alpha = 0{\cdot}025$ given by Piddington and Minnett[8] for the micro-wave attenuation coefficient α, the difference between the mean temperatures deduced from micro-wave and optical observations should be 135 deg. C. This figure is probably far too low, since the value of α used is of the order of that for dry soil and the attenuation for lunar surface material should be much lower. This calculation provides an independent indication that the temperatures deduced by Fremlin are much too high. But there is some evidence that the mean micro-wave temperature is a few degrees higher than the mean optical temperatures and a careful analysis might lead to information about the lunar heat flux.

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Prof. Jaeger is quite right in pointing out that I have used a high figure for the radioactivity of the Moon, and also that my value for conductivity near the surface is lower than the mean value calculated from observations. This does not necessarily prove that my values are wrong. I assume Kozyrev's observations of hot gases to be reliable. They would not have been expected on the basis of conventional views of lunar characteristics and presumably, therefore, conventional views must be modified. I have suggested some modifications, which may easily be wrong. The more conservative of Prof. Jaeger's figures, however, must almost necessarily be wrong. They show that phenomena observed by Kozyrev could not have occurred.

I do not think that there is a contradiction between my figures for conductivity and experiments done so far. My whole argument is based on the idea of large thicknesses of dust in specific limited areas. Measured radiation from the lunar surface has been averaged from considerable areas (in the micro-wave work, nearly the whole of the Moon's disk) within which any bare rock would be expected to have a thermal conductivity very much higher than the mean value observed. Small areas of high conductivity could have a large effect on the calculated average temperatures on the dark side of the Moon. Their increasing share of the total radiation at the lower average temperatures might quite possibly explain the small divergence shown between Pettit's infra-red observations during a lunar eclipse and Prof. Jaeger's calculations (his reference 9, Fig. 1). These calculations show the apparent variations of temperature with time to be expected for a uniform lunar surface with values of $(K\rho c)^{-1/2}$ from 20 (bare rock) to 1,370. The calculated temperatures fall too fast in the umbral phase. Instead of assuming inhomogeneity with depth, let us assume the existence of two types of surface, one bare rock ($K\rho c^{-1/2} = 20$) and one a dust ($K\rho c^{-1/2} = 2{,}700$) of about five times lower conductivity than the mean value giving best agreement. Then we can get an almost exact fit with Pettit's observations if the bare rock covers 4·8 per cent of the total area—a not unreasonable result. (The two volumes of conductivity are quite arbitrary, having been chosen simply to make computation from Prof. Jaeger's calculations as easy as possible.) If we do not limit ourselves to two kinds of surface only, or if we allow for a difference of albedo, there would be no difficulty in accommodating adequate areas of conductivity well below even those assumed in my original communication.

It is in any event difficult to believe that the alternative picture suggested by Piddington and Minnett, of a uniform 1-mm. layer of dust, over mountain-side and plain alike, can be more than a convenient analytical model.

With regard to the supposed radioactivity, I am on weaker ground. I had failed to consider the need for high mechanical strength to maintain the observed shape of the Moon, and I am grateful to Prof. Jaeger for pointing this out. It is clear that the average lunar radioactivity must be well below that given for the Earth's crust, unless the thermal conductivity of the solid lunar rock is a good deal higher than hitherto supposed. The obvious way out is to suggest a non-uniform distribution of radioactive material. The likelihood of this depends on what we assume about lunar history, which seems too large a subject to discuss here.

My main contention remains: that I am willing to feel doubt about any of my assumptions, but am unwilling to give up the entire picture until an alternative explanation of Kozyrev's observation is given.

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Magnetostriction and Palæomagnetism of Igneous Rocks

In a recent communication, Stott and Stacey[1] report on a "crucial experiment" from which they conclude: "This excellent agreement between the dip and the directions of artificial thermoremanent magnetization of the stressed and unstressed rocks indicates that large systematic errors due to magnetostriction are most improbable in igneous rocks of types normally used for palæomagnetic work". This experiment was intended to test the proposals[2] and measurements[3] bearing on the role of magnetostriction in rock magnetism. We present here our reasons for believing that the experiment was not crucial and that the conclusion is not justified.

The magnetic fraction of natural igneous rocks almost without exception consists of mixtures of two or more crystalline phases. These phases may be distinguished magnetically by a number of parameters which are dependent on structure, composition and particle-size. Commonly these segregated phases are intimately intermingled, for they developed from pre-existing simpler phases which were stable only at a higher temperature. As a result, there may be strong interaction of the magnetic fields of the different phases on one another. The magnetic properties of the bulk samples are thus determined both by the properties of the individual phases and by the nature of the magnetic interactions among them. The degree to which the phases have become segregated and the size scale on which the intergrowth textures have developed depend, among other things, on composition and thermal history. Kawai *et al.*[4] have demonstrated that in Japanese effusive rocks, unmixing at low temperatures is a process requiring tens of millions of years, whereas, in contrast, low-temperature unmixing can be detected in as little as three days in a particular sample which was first held at 750° C. for three days and was then quenched to room temperature. As a result of unmixing, the magnetic properties of rocks change conspicuously. At elevated temperatures, the rate of homogenization of unmixed phases varies greatly among different samples; heat treatment at 650° C. for 15 min. is sufficient to cause gross changes in the phase structure and composition of the magnetic fraction of numerous rocks. This fact has been observed repeatedly in many laboratories and is abundantly documented, especially in the Japanese works.

Stott and Stacey ignore these facts, assuming that their experiments on natural samples which have been heat-treated 15 min. at 650° C. and then cooled over a period of several hours while under axial stress give realistic replicas of typical conditions in Nature. Clearly, this assumption is not safe, except possibly for the case of some young rocks which were cooled quickly; it does not permit generalizations, for example, about the possible role of magnetostriction in influencing the directions of residual magnetization of samples of a dolorite sill which cooled over a period of hundreds of years while under load, remained under a load that decreased over millions of years while magnetic phases were segregating from original simpler phases, and finally were brought to the laboratory for measurement.

We feel that this oversight by Stott and Stacey should be pointed out for, in our opinion, there are already too many instances where conclusions founded improperly have given the subject of palæomagnetism a questionable status.

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Borides and Silicides of the Platinum Metals

A great number of intermediate phases exist in the systems of boron with platinum metals[1] but very little information about these phases is available. X-ray crystallographic studies of platinum metal borides have been initiated at this Institute, and we have determined three new structure types (tentatively called Ru_7B_3, RhB and IrB_{1-2} types) which will be discussed briefly in this communication. We have found evidence that the 'Rh_2B' phase with *C*23 structure reported by Mooney and Welch[2] might really be Rh_2Si.

The alloys were prepared by melting mixtures of the elements (99·7–99·8 per cent pure) in an arc furnace. The crystal structures were determined with single-crystal methods, while the lattice parameters were obtained from powder photographs.

The Ru_7B_3 structure. This structure is hexagonal and was recently described in detail[3]. The space group is $P6_3mc$ with two formula units in the unit cell. Ru_7B_3 and Rh_7B_3 crystallize in this structure, which is especially interesting for its close similarity to the Cr_7C_3 structure[4].

The RhB structure. The most probable space group of this orthorhombic structure is *Cmcm*. The four

Table 1. Crystallographic Data of Some Borides and Silicides of the Platinum Metals

Substance	Space group	Lattice parameters (A.)
Ru_7B_3	$P6_3mc$	$a = 7{\cdot}46_7$, $c = 4{\cdot}71_2$
Ru_2B_3	$P6_3mc$	$a = 7{\cdot}47_3$, $c = 4{\cdot}77_4$
RhB	$Cmcm$	$a = 3{\cdot}30_3$, $b = 5{\cdot}68_7$, $c = 4{\cdot}34_3$
PtB	$Cmcm$	$a = 3{\cdot}35_6$, $b = 5{\cdot}80_9$, $c = 4{\cdot}06_2$
IrB_{1-2}	$I4_1/amd$	$a = 2{\cdot}81$, $c = 10{\cdot}27$
Ru_2Si	$Pnma$ (C23 type)	$a = 5{\cdot}27_9$, $b = 4{\cdot}00_3$, $c = 7{\cdot}41_8$
Rh_2Si	$Pnma$ (C23 type)	$a = 5{\cdot}40_8$, $b = 3{\cdot}93_0$, $c = 7{\cdot}38_2$

rhodium atoms of the unit cell are situated in 4(*c*) with $y = 0{\cdot}167$. The boron atoms occupy the 4(*b*) position, which corresponds to octahedral holes in the metal skeleton. The final *R*-value of the 45 observed 0*kl* reflexions was 8·9 per cent. PtB is isomorphous with RhB.

The IrB_{1-2} structure. In iridium–boron alloys around the equiatomic composition a phase with simple structure was observed. Since from structural reasons the ideal composition of this phase probably is IrB_2 it will be denoted IrB_{1-2}. The space group is $I4_1/amd$ with four iridium atoms in 4(*a*). With this arrangement of the metal atoms an *R*-value of 9 per cent was obtained for the thirty observed *h*0*l* reflexions with $\sin\theta/\lambda > 0{\cdot}8$. Eight boron atoms can be accommodated in 8(*e*) with $z \sim 0{\cdot}42$, but this position is probably only partly occupied. The boron atoms have the same environment as those in the AlB_2 structure.

Borides and silicides of the C23 *type.* We have found that intermediate phases of the *C*23 type exist in the Ru–Si and Rh–Si systems, but we could not confirm that 'Rh_2B' has this structure. It does not seem unlikely that the 'Rh_2B' crystals studied by Mooney and Welch[2] in reality had the composition Rh_2Si. We would like to put forward the following arguments to support this suggestion. (1) The ability of amorphous boron to reduce silica has been pointed out already by Moissan[5], and recently[6] it has been shown that nickel–boron samples easily react with silica to form Ni_6Si_2B. Thus, it is not unreasonable to assume that also rhodium–boron samples are able to react with silica to form alloys containing silicon. (2) The lattice parameters reported for Rh_2B ($a = 5{\cdot}42$ A., $b = 3{\cdot}98$ A., $c = 7{\cdot}44$ A.) are within experimental errors equal to (or slightly larger than) those of Rh_2Si (see Table 1). If 'Rh_2B' and Rh_2Si were isomorphous, one would expect the unit volume of the former to be smaller than that of the latter. (3) The experimental density of the 'Rh_2B' alloy of Mooney and Welch[2] was 10·5 gm./cm.^{3}, which agrees better with the calculated value for Rh_2Si (9·90 gm./cm.^{3}) than with that for 'Rh_2B' (8·96 gm./cm.^{3}). (4) The published intensity data for 'Rh_2B' are compatible with our suggestion. If the calculated *h*0*l* structure factors for Ru_2Si (which we have refined from single-crystal data) are compared with those observed from 'Rh_2B', an *R*-value of 0·21 is obtained, whereas the value of 0·26 was reported for 'Rh_2B'. (5) Finally, the average Rh–B distance in 'Rh_2B' is 2·50 A., which is much longer than one would expect for metal–boron contacts (in Ru_7B_3 the average *Me*–B distance is 2·16 A., in RhB 2·19 A.), whereas 2·50 A. is a very reasonable Rh–Si distance.

It is also possible that the reported Pd_3B_2 phase[1] might have been Pd_2Si (perhaps with dissolved boron), which crystallizes in the revised (hexagonal) *C*22 structure[6,7].

We wish to thank Prof. G. Hägg for his kind interest and the European Commission of Air Research and Development Command for financial support (Contract No. *AF*61(052)-40).

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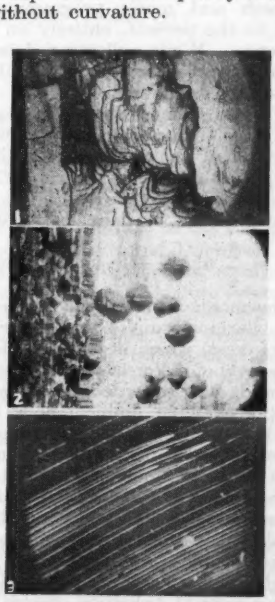
Slip Sources on Sodium Chloride Surfaces

Many experiments have been carried out with sodium chloride crystals to investigate the Joffé effect, according to which the dissolution of surface layers by surrounding solvents results in an improvement of the plastic properties of the crystal. Hollomon and Fisher[1] suggested that this might be due to surface sources of slip which play an important part during the initiation of plastic flow.

Sodium chloride crystals were grown from a melt and their surfaces investigated with an opaque illuminator microscope. A set of small lines was observed. The crystals were then put in a not quite saturated aqueous solution of sodium chloride, which acted as an etching reagent. After a few minutes of etching the solution was made supersaturated by temperature regulation so that a slow growth started on the etched surface. A typical result is shown in Fig. 1. It is believed that the etching revealed slip sources which then became amplified by the subsequent epitaxy from the supersaturated solution. The curved lines may be parts of Frank–Read sources, which usually cover the surfaces of sodium chloride crystals grown from a melt[2]. On surfaces which do not contain slip sources the epitaxy reveals a cubic structure without curvature.



Figs. 1–3. (× 125)

Fig. 2 shows a specimen which was etched for a few minutes in 80 per cent alcohol. Etch pits became visible. On the left the surface is crowded with pits, on the right the pits are better resolved. As the pits are pointed they may be dislocations[2]. The left side seems to be the result of a crack passing slowly through the crystal. Such cracks are known to create a large number of dislocations at their edge.

If the crystals are heated to a high temperature (600° C.) and quenched, discontinuous lines become visible after etching. Fig. 3 shows a typical example on a dark field. It is suggested that the discontinuous lines consist of dislocations piled up against some obstacle.

If the surfaces are dissolved in water the number of observable slip sources is considerably reduced.

This work was carried out in the Institute for Experimental Physics of the University of Technology, Budapest. I wish to thank B. Jeszenszky for valuable discussions. A detailed paper will be published in *Acta Physica Hungarica*.

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Budapest. Mar. 2.

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Unpleasantness of Distorted Sounds : a Criterion derived from the Distortion Spectrum

For lack of any proved method of objective assessment the degree of non-linear distortion permissible in transmission systems which handle high-quality speech and music programmes has been decided, up to the present, entirely on the basis of long experience. While collecting data to support a more factual approach to this subject the following experiments have been made.

Pulses of pure tone, rather like staccato piano notes, have been distorted in various ways and the 'unpleasantness' of the resulting sounds have been compared, and an objective criterion, C_t, computed. This shows satisfactory correlation with the unpleasantness and can be defined as follows :

$$C_t = \sum_{2}^{n} n^3 (p_n - t); \quad (p_n - t) \text{ positive}$$

where n is harmonic number, p_n is percentage amplitude of nth harmonic and t that percentage of the signal (at its crest) which is just audible.

When the level of the crest of the pulse is 70 phons, and the sounds are presented under good listening conditions (for example, ambient noise in the region of 30 phons) a value of $t = 0{\cdot}2$ gives good correlation with subjective assessment. It is suggested that $t = 0{\cdot}5$–$1{\cdot}0$ may be more appropriate so far as domestic broadcasting is concerned. Since t is that harmonic percentage which is just audible, the value of t will fall below these figures if the signal/noise ratio is increased, or rise if the ratio is reduced.

The validity of the criterion C_t has been tested by setting up pairs of distortion spectra, which differ radically both in their general character and in their band-width, and adjusting one of the spectra until the two sounds are equally unpleasant. If the signal frequency lies in the range 250–1,500 c./s., the value of the criterion C_t derived from each spectrum will then be substantially the same, provided the appropriate value of t is adopted ; anomalies occur outside this frequency-range.

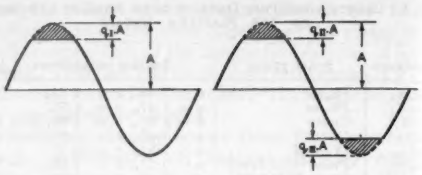

Fig. 1

A form of distortion which is easily adjusted and controlled is shown at I in Fig. 1 ; the proportion q_I of the amplitude, A, of the sine-wave can be varied by adjusting the steady bias applied to a diode shunting the signal path. The criterion C_t increases roughly as $\sqrt{q}$, when t is made very small or zero. Distortion of the type shown at II in Fig. 1 is often found, although in a form very considerably attenuated by negative feedback, in high-power (for example, 150 kW.) modulating-amplifiers for amplitude-modulated radio transmitters ; in this case both positive- and negative-going parts of the wave are limited. The correlation between the criterion C_t and the unpleasantness of both types of distortion has been investigated, and a report will be published in due course elsewhere.

The outcome of one set of experiments is as follows : it is found that when $q_I = q_{II}$, and q is in the range $0{\cdot}5 > q > 0$ the signal I in the figure is (within the limits of experimental error) as unpleasant as the signal II (although there is a great difference in tone-colour). On comparing the spectra the two criteria are found to differ by less than 10 per cent when t is made zero and at least eight terms are summed.

Now when $q_I = q_{II}$ the spectrum of the signal II is similar to the spectrum of I, except that all the odd terms are doubled in amplitude and the even terms are entirely absent. Thus on summing the terms to derive the criterion, the absence of half of them is made up for by the doubled amplitude of those remaining.

It is important to notice that roughly the same effect would have been obtained if n or n^2 were substituted for n^3 in the equation ; that the signals I and II sound equally unpleasant, therefore, provides no direct test of the criterion. However, it does demonstrate that any criterion (such as the total harmonic percentage) which is based upon the summation of terms in $(p_n)^2$, must necessarily fail to correlate with the unpleasantness.

There is some evidence that $\log(C_t)$ may be used to predict the 'rating' of the distortion ; thus, if the rating-scale is expressed by a series of numbers such as : 1, 'distortion not perceptible' ; 2, 'perceptible with effort' ; 3, 'perceptible without effort' and so on, the rating number tends to vary with $\log(C_t)$.

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CHEMISTRY

Smoke Emission of Low-Temperature Chars

In a recent communication by Adams, Gaines, Gregory and Pitt[1], the authors make the usual assumption that coal contains a material of character such that it is partly removed, as tar, on heating. I wish to offer an alternative suggestion.

It is generally admitted that a great part of bitumin coal substance has a polymethylene structure.

It is a well-known fact that the early products of the carbonization of coal are ethane, ethylene, hydrogen, methane and tar.

Now in a series of papers summarized and extended by Dr. C. G. Silcocks and myself in two recent papers[2], we show that ethane and ethylene react at temperatures in the region in which low-temperature carbonization is usually effected, thus:

$$C_2H_4 + C_2H_6 = C_4H_8 + H_2 \quad (1)$$

$$C_2H_4 + C_2H_4 = C_4H_8 \quad (2)$$

The product, in the presence of excess hydrogen, is converted mainly into methane, and, in the absence of hydrogen, it loses hydrogen and polymerizes, forming tarry bodies. Of the nature of these strange bimolecular reactions I can only say that they appear to be homogeneous *in vitro*.

In a recent communication published in the *Journal of the Institute of Fuel* (April 1957) I suggested that this combination of reactions may account for the formation of methane and tar during the process of low-temperature carbonization of coal and, if this is the case, the formation of tar may continue so long as any methylene structure remains in the coal. *In vitro* the bimolecular reactions (1) and (2) appear to be homogeneous, but what may happen when C_2-hydrocarbon is formed from the methylene structure within the mass of the coal is unpredictable.

The bimolecular reactions (1) and (2) are unique in that propane and propylene behave in no way similarly. But during the pyrolysis of higher hydrocarbons, C_2-hydrocarbons are always formed; then further changes follow as indicated.

In 1938, I communicated to the Faraday Society a paper on the system:

$$\text{carbon} + \text{hydrogen} \rightleftharpoons \text{methane}$$

At temperatures below 700° C. the equilibrium relations appeared to indicate that methane was formed by a reaction represented by:

$$\rangle CH_2 + H_2 \rightleftharpoons CH_4$$

The work was made difficult by the fact that equilibrium was established slowly, and leakage of hydrogen through the walls of the reaction vessel had to be allowed for. By itself I should not have paid much attention to this result, but it falls into line with the other experimental work I have mentioned.

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Effect of Carrier Gas on Katharometer Response

In recent communications[1] the relative effects of gases of high and low thermal conductivity on the response of katharometers to the presence of eluted substances have been considered. Several recent publications[2-5] have dealt with this, but a few points can usefully be emphasized.

The classical heat balance of a katharometer is given by[6]:

$$\frac{i^2R}{J} = Ctk_1 + H \quad (1)$$

where i is the filament current, R its resistance, C a cell constant, k_1 the thermal conductivity of the gas, and t the difference in temperature between the wire and the walls of the cell. H represents the heat lost by a variety of means such as convection, conduction down the leads, radiation and transfer to a flowing gas. This has been passed over by Ray and by Schmauch and Dinerstein but is of great importance, and the last factor particularly is important in katharometer behaviour, as shown by Bohemen and Purnell[2].

The sensitivity of a katharometer is measured in electrical units. Thus, a change dt in the temperature of the wire produces a change dR in its resistance and hence a change in potential of $i\,dR$ volts. Ignoring H, as above, it can then be shown[3] that:

$$(dE)_t = \frac{J\alpha Ct^2}{i}\,dk_1 \text{ (volts)} \quad (2)$$

where $(dE)_t$ is the katharometer response at $t°$ C. and α is the temperature coefficient of resistance of the wire.

Whence, from (1):

$$(dE)_t = \alpha.\frac{i^2R^2}{CJk_1^2}\,dk_1 \quad (3)$$

However, also from (1) and (2):

$$(dE)_t = \alpha\,\frac{(JRt^2)^{1/2}}{(Ck_1)^{1/2}}\,dk_1 \quad (4)$$

Equation 4, which has been verified experimentally[3], applies, of course, only to one carrier gas, but is interesting in that should equation 3 be generally true, as argued by Ray, the response of katharometers at constant temperature of the wire is inversely proportional to $k_1^{1/2}$, in contrast to the statements both of Ray and of Schmauch and Dinerstein. Furthermore, there is evidence[3] supporting equation 4 which fits the facts of hydrogen and helium systems remarkably well. However, this is purely fortuitous and due to the fact[1] that dk_1/dx is approximately equal for these two gases. If the thermal conductivity (k_2) of a diluent is very much less than that of the carrier then $(k_1 - k_2)/k_1$ or dk_1/k_1 should be approximately equal for two carriers. Whence the ratio of helium to hydrogen responses becomes:

$$\frac{(dE)\text{He}}{(dE)\text{H}} = \sqrt{\frac{k_1\text{He}}{k_1\text{H}}} \quad (5)$$

a result which simply does not fit the facts. It has been found necessary to qualify the significance of $(k_1 - k_2)/k_1$ by the introduction of constants only approximately unity, thus: $(b_1k_1 - b_2k_2)/k_1$. I under-

stand that there is theoretical justification for this (Littlewood, A. B., private communication). When considering other carrier gases the importance of H (equation 1) cannot be ignored as it can with hydrogen and helium. This greatly complicates the mathematics, and systems worth studying would be helium in hydrogen and water vapour in both, where k_1 and k_2 are nearly equal but H can be ignored.

A final point not mentioned by Ray is that the temperature of the wire rises when the working substances he uses are eluted in helium, but falls when they are eluted in argon. This has been missed by Schmauch and Dinerstein, who give responses only for substances which cause a rise in the temperatures of the wire with nitrogen. The results are not, therefore, comparable. In any event, the point is a simple one. Helium is obviously much more easily detected by katharometer in nitrogen or argon than in hydrogen.

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Use of Katharometers in Gas Chromatography

In a recent communication[1], Ray attacks the tenet that "the use of hydrogen or helium as the carrier gas in gas chromatography gives the highest sensitivity with a thermal conductivity detector". He derives an equation describing katharometer behaviour from which he infers that the carrier gas with the lowest thermal conductivity will give the highest sensitivity. He concludes that nitrogen, argon, or carbon dioxide will provide greater sensitivity than hydrogen or helium. We wish to point out that the conclusion drawn from this equation does not follow generally, although it may be valid in certain instances. Furthermore, a basic assumption in Ray's derivation is contrary to the usual operating procedure for katharometers.

His equation, $dt/dx = -a(k_1 - k_2)/k_1^2$, states that the sensitivity of the detector, dt/dx, is proportional to the difference between the thermal conductivity of the carrier gas, k_1, and the vapour, k_2, and inversely proportional to the square of the thermal conductivity of the carrier gas. As Ray implies, this function has an arbitrarily large negative value as k_1 approaches zero. He fails to note that it also exhibits a maximum when the thermal conductivity of the carrier gas is twice that of the vapour being analysed.

We have computed values (Table 1) of katharometer sensitivity using this equation and the values of thermal conductivity for various carrier gases and organic vapours given in the International Critical Tables[2] for 0° C. It will be seen that argon provides high sensitivity for methane, low sensitivity for ethylene and again high sensitivity for benzene. This is because in the first instance the thermal conductivity of argon is approximately one-half that of the methane; in the second instance, the thermal conductivities of carrier and vapour are nearly identical, and in the third instance the thermal conductivity of argon is almost exactly twice that of benzene. Thus, the response of the katharometer may be either positive or negative depending on whether the organic vapour being detected has a smaller or larger thermal conductivity than the carrier, and also varies greatly in absolute value. In contrast, helium, the thermal conductivity of which is high relative to organic vapour, gives a nearly constant sensitivity, which permits quantitative estimation of the concentration of each component in a complex mixture.

Table 1. Thermal Detector Sensitivities Relative to Methane in Helium

	Argon ($k = 1{\cdot}58$)	Nitrogen ($k = 2{\cdot}28$)	Helium ($k = 13{\cdot}9$)
Methane ($k = 2{\cdot}94$)	−9·82	−2·11	1·00
Ethane ($k = 1{\cdot}80$)	−1·58	1·61	1·11
Ethylene ($k = 1{\cdot}64$)	−0·44	2·11	1·12
m-Pentane ($k = 1{\cdot}16$)	2·98	3·86	1·16
Benzene ($k = 0{\cdot}825$)	5·44	4·91	1·19

Table 2. Constant Hot-Wire Temperature

	Argon	Nitrogen	Helium
Methane	−1·09	−0·35	1·00
Ethane	−0·18	+0·27	1·10
Ethylene	−0·05	0·35	1·12
m-Pentane	0·34	0·62	1·15
Benzene	0·59	0·81	1·19

It should also be noted that Ray's equation 1 implies that in all experiments the same current flows in the detector. However, it is common practice to increase the current when using a carrier gas of higher thermal conductivity, so that the difference in temperature between hot wire and cell wall is kept constant. Under these conditions the approximate equation will be $dt/dx = -a(k_2 - k_1)/k_1$.

Relative sensitivities (Table 2) have been computed for this mode of operation. It is clear that helium is eminently superior to either argon or nitrogen. Thus, while it may be true that the use of a carrier gas of thermal conductivity much lower than that of any organic component to be analysed would exceed helium in sensitivity, no gas now available is applicable to the large variety of substances at present being analysed by gas chromatography.

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Effect of the Carrier Gas on the Sensitivity of Thermal Conductivity Detectors in Gas Chromatography

It has recently been suggested[1] that the use of argon or other carrier gases of low thermal conductivity will lead to increased sensitivity of thermal conductivity detectors used in gas chromatography. While this may be so in certain special cases, it is by no means a general effect, and it is the purpose of this communication to point out some of the many disadvantages of carrier gases of low thermal conductivity. The effects we wish to discuss may be

illustrated qualitatively by the use of the approximate equation[1] for the variation of filament temperature (t) with composition (x) of the eluted gas.

$$\frac{dt}{dx} = \frac{a(k_1 - k_2)}{k_1^2} \quad (1)$$

where k_1,k_2 are the thermal conductivities of the carrier gas and organic vapour respectively. Substituting the thermal conductivity data from Table 1 in equation 1 leads to the predicted relative sensitivities shown in Table 1. The substances investigated, benzene, *n*-heptane, chloroform, carbon tetrachloride and methyl chloride, would appear from Table 1 to show a higher sensitivity with argon than with helium but ethanol would show a lower sensitivity, and methanol would show a peak reversal compared to ethanol. It should be pointed out that these estimates are only crude approximations since the initial temperature of the detector filament is unknown and is probably considerably higher than 100° C. for our apparatus in which the block temperature is 100° C.

Table 1. Thermal Conductivities of Gases and Vapours at 100° C. (Ref. 2)

Substance	k cal. cm.$^{-1}$ sec.$^{-1}$ deg.$^{-1}$	$\frac{k_1 - k_2}{k_1^2} \times 10^3$	
		Helium	Argon
Helium	39·85	—	—
Argon	5·087	—	—
Chloroform	2·333	2·36	+10·64
Carbon tetrachloride	2·048	2·38	+11·74
Methyl chloride	3·841	2·27	+ 4·81
Ethanol	4·99	2·20	+ 0·41
Methanol	5·161	2·18	− 0·28

Ideally, comparison of sensitivities for different carrier gases should be made with constant filament temperature rather than constant current. Such a comparison would lead to different relative sensitivities and generally lower sensitivities for argon relative to helium than those obtained in our experiments. Unfortunately, our experimental arrangement did not allow measurements of potential across the filament and therefore comparisons were made with the same filament current. This does not, however, vitiate our qualitative conclusions.

A Perkin–Elmer *F* column containing 25 per cent by weight of stationary phase was used at 100° C. with a flow-rate of 100 cm.^{3}/min. and a constant filament current of 140 m.amp. Results were obtained with various mixtures using a 'Gow-Mac' four-filament type thermal-conductivity cell as shown in Table 2.

Table 2 shows that the relative responses for various substances are more nearly equal with helium than with argon as carrier. In the first four systems recorded, the mixtures contained equal volumes of the two components. The sign of deflexion is taken relative to the ethanol peak; thus we see that for

Table 2

Mixture	Sensitivity ratio EtOH/x	
	Helium	Argon
MeOH + EtOH	+0·86	+0·63
EtAc + EtOH	+1·19	+9·00
CCl_4 + EtOH	+1·15	−1·11
$CHCl_3$ + EtOH	+1·69	−1·78
CH_3I + EtOH	+0·65	−0·57
CH_3Cl + EtOH	+4·45	+ 126

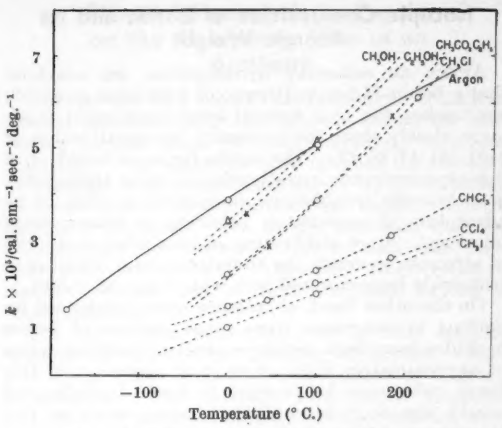

Fig. 1. Thermal conductivities of gases and vapours

three of the systems the peaks are in the same direction as ethanol in argon and in the remaining three the peaks are reversed in argon. In fact, the ethanol peak is reversed in argon relative to helium. Secondly, we see that for some mixtures the detector is less sensitive in argon.

If a vapour possessed the same thermal conductivity as the carrier gas at the effective temperature (which is not necessarily the filament or the block temperature), then the detector would be insensitive during the passage of eluted vapour through the conductivity cell. Such is almost the case with methyl chloride with a value of + 126 for the sensitivity ratio in argon shown in Table 2. This affords an approximate method of determining the effective temperature, which from Fig. 1 is around 260° C. Unfortunately there is no such obvious method of deducing the temperature with helium, although the temperature will be lower than in argon for any given current. Comparison with Fig. 1 shows that the sensitivity of systems exhibiting peak reversal in argon correlates with the distance from the argon thermal conductivity line.

The above results confirm the qualitative conclusions drawn from equation 1, that the variation of sensitivity when argon is used as carrier gas is large, varying from large positive sensitivities through zero to large negative sensitivities, whereas the sensitivity when helium is used as carrier gas is more nearly constant for a range of materials. Since gas chromatography is very often used to analyse for materials the nature of which is unknown and since the data on thermal conductivities are sparse, it would seem that helium or hydrogen is a better carrier gas to use for general purposes and that argon or other gases of low thermal conductivity should be used only in cases where calibration has shown that the ultimate sensitivity is substantially higher.

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Isotopic Composition of Boron and its Atomic Weight

After an extensive investigation, we conclude that a boron-11/boron-10 ratio of 4·00 (that is, 80/20) approaches the true natural abundance ratio much more closely than the presently 'accepted' value of 4·31 (81·17/18·83). The latter figure is based on a mass spectrometric investigation of boron trifluoride[1], a notoriously troublesome compound, in view of its adsorption characteristics resulting in instrumental 'memory'. Since publication of that work a number of attempts to verify the abundance ratio from boron trifluoride have resulted in a wide range of values.

On the other hand, abundance ratios calculated by various investigators from mass spectra of boron hydrides have been highly consistent, yielding ratios of approximately 4·00. Lest it be argued that this lower value may be peculiar to boron hydrides, we would like to point out that recent work in this laboratory with other compounds (for example, trimethylboroxine) has yielded the same ratio.

We therefore propose that in the future it would be far more reasonable to use the ratio 4·00 (80/20) to represent the natural isotopic abundance of boron. This would require in addition a revision of the atomic weight from 10·82 to 10·811, a change which seems well within the range of experimental error of earlier investigators[2].

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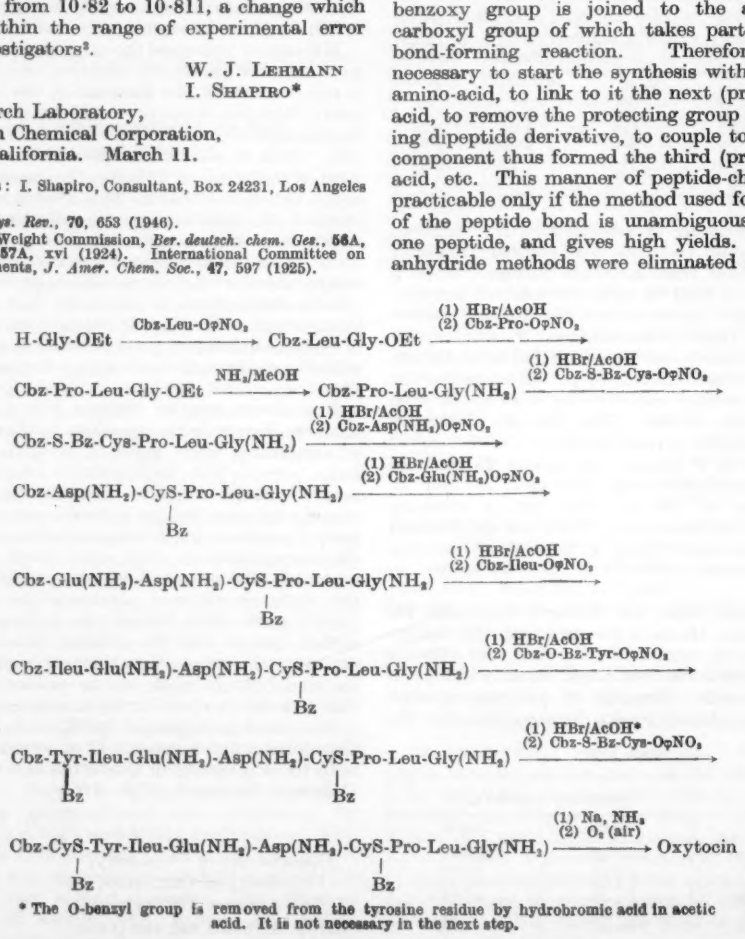
BIOCHEMISTRY

Synthesis of Oxytocin by the Nitrophenyl Ester Method

After the completion of work on the degration of oxytocin[1], this compound was synthesized in this laboratory[2]. Since that time several other synthetic methods leading to biologically active solutions of the crude hormone have been reported[3]. Recently, an improved synthesis of oxytocin was elaborated[4] by us. All these syntheses of oxytocin have the common feature of linking peptides together. It is well known, however, that formation of the peptide bond with a protected peptide as the acid component can lead to racemization[5], the extent of which depends on the components, the conditions and the method. No racemization was observed with the use of the azide method[6]. Nevertheless, the latter procedure can involve the formation of by-products other than diastereoisomers. To avoid racemization it is possible to take advantage of the protecting groups, for example, the carbobenzoxy group, which protects the amino-acid acylated by it not only against unwanted acylation but also against racemization. This protection is effective only if the carbobenzoxy group is joined to the amino-acid, the carboxyl group of which takes part in the peptide bond-forming reaction. Therefore it becomes necessary to start the synthesis with the C-terminal amino-acid, to link to it the next (protected) amino-acid, to remove the protecting group from the resulting dipeptide derivative, to couple to the free amino component thus formed the third (protected) amino-acid, etc. This manner of peptide-chain synthesis is practicable only if the method used for the formation of the peptide bond is unambiguous, leads to only one peptide, and gives high yields. All the mixed anhydride methods were eliminated as they usually



* The O-benzyl group is removed from the tyrosine residue by hydrobromic acid in acetic acid. It is not necessary in the next step.

Fig. 1

lead to two acylation products, the separation of which, for example by crystallization, can be difficult, especially in cases of peptides with several amino-acids. After the elimination of many other unsuitable possibilities the nitrophenyl esters of acylamino-acids were found to be suitable for this purpose.

The use of acylamino-acid nitrophenyl esters in peptide synthesis was suggested and applied earlier by one of us (M. B.)[7]. An improved method for the preparation of such esters was proposed later by Schwyzer and his co-workers[8]. Recently, however, dicyclohexylcarbodiimide was used by Elliot and Russel[9] for the esterification of acylamino-acids with *p*-nitrophenol and we have found this procedure to be very convenient, giving high yields of pure products. For the synthesis of oxytocin, the *p*-nitrophenyl esters of cbz-L-leucine, cbz-L-proline, S-benzyl-N-cbz-L-cysteine, cbz-L-asparagine, cbz-L-glutamine, cbz-L-isoleucine and O-benzyl-N-cbz-L-tyrosine were prepared. All these esters are stable, optically pure compounds with excellent crystallization properties. The synthesis of oxytocin with the aid of these esters is outlined in Fig. 1.

Lengthening the already existing chain by one amino-acid was achieved in very satisfactory yields, usually 90–100 per cent of the theoretical amount. The protected peptides were obtained in reasonable purity and gave good analyses even without further purification. Most of the intermediates were readily obtained in crystalline form. The purity of the protected nonapeptide is best demonstrated by the fact that on removal of the protecting groups by sodium in liquid ammonia, followed by aeration, oxytocin was formed in 78 per cent yield from it. Experimental details of this work will be presented elsewhere.

We believe that the method of synthesis outlined here may be useful both for structural proof and also for practical preparation of larger peptides of biological interest.

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Effect of Hydrolysis by Papain on the Combining Sites of an Antibody

PORTER[1] recently found that proteolysis of each of several different rabbit antiprotein antibodies with papain yielded three chromatographically separable fractions. Two of the fractions retained the capacity to combine with antigen, as shown by their ability to block specific precipitation, but would not form specific precipitates. The results indicated that the bivalent antibody molecule had in each case been split into two univalent fragments which retained specific activity and a third fragment lacking a combining site.

The use of anti-hapten antibody permits determinations of the specific binding of hapten, which provide a direct measure of the effect of a given treatment on the average equilibrium constant and the number of combining sites. In contrast with the precipitin reaction, quantitative measurements can be made with only one active site present on a molecule or fragment. With this technique we have found that hydrolysis of the antibody molecule by crystalline papain into fragments about one-third the original size has little effect on the capacity to bind hapten; that is, the combining sites in the degraded molecules are almost completely intact.

Experiments were done with a γ-globulin fraction of rabbit anti-*p*-azobenzoate antibody (containing 6·1 per cent of antibody specifically precipitable by an ovalbumin-*p*-azobenzoate test antigen[2]) and also with specifically purified antibody (73 per cent precipitable). Binding measurements were carried out by equilibrium dialysis with radioactive *p*-iodobenzoate having a radiochemical purity greater than 99 per cent. Methods used for the preparation of specifically purified antibody and for equilibrium dialysis have been described in detail[2,3].

Preparations of γ-globulins were made by the procedure of Kekwick[4] and were found to migrate as a single peak (6·5–7 S.) in the ultracentrifuge. Papain (twice crystallized) was purchased from the Worthington Company.

Antibody or normal γ-globulin preparations were treated at *p*H 7 with the amounts of papain and in the buffer indicated in Table 1. Mixtures were allowed to stand for 18–20 hr. at 37° C., then at room temperature. During the next 6 hr., measurements were made in the ultracentrifuge; equilibrium dialysis was then carried out with portions of the same samples.

Results are given in Table 1. For the samples treated with papain the $S_{20,w}$ values fall in a narrow range (3·4–3·6 S.) as compared with 6·5–7·0 S. for the untreated antibody γ-globulin or normal γ-globulin. None of the faster-moving material (6·5–7 S.) remained in any of the preparations treated with papain. In the case of antibody γ-globulin the amount of papain used was varied by a factor of 10. Thus the use of more enzyme resulted in little change in the apparent sedimentation constant, although the fraction of dialysable protein increased from 13 per cent (sample 4*A*) to 42 per cent (sample 6*A*). The $S_{20,w}$ values for the hydrolysed samples are in close agreement with those found by Porter[1], who observed a single peak with $S_{20,w} = 3{\cdot}5$, and a molecular weight of 55,000–60,000 (as compared with 160,000 for untreated antibody). The small amounts of papain ($S \simeq 2{\cdot}4$)[5] used did not appear in the sedimentation patterns as a separate peak.

Table 1. Effect of Crystalline Papain on Rabbit Anti-*p*-Azobenzoate Antibody*

Experiment No.	Sample	Papain used (mgm.)	$S_{20,w}$†	Conc. labelled *p*-iodobenzoate bound‡ ($M \times 10^6$)
1*A*	Normal γ-globulin	0	6·7	—
2*A*	Normal γ-globulin	16·5	3·4	0·06 ± 0·03
3*A*	Antibody γ-globulin	0	6·5	2·91 ± 0·07
4*A*	Antibody γ-globulin	1·7	3·6	2·66 ± 0·07
5*A*	Antibody γ-globulin	4·2	3·5	2·58 ± 0·03
6*A*	Antibody γ-globulin	16·5	3·5	2·17 ± 0·04
1*B*	Specifically purified antibody	0	7·0	5·07 ± 0·03
2*B*	Specifically purified antibody	0·5	3·5	4·75 ± 0·06

* Hydrolysis at 37° C. for 18–20 hr.; *p*H 7·0. Buffer contained sodium salts of phosphate (0·1 *M*), versene (0·002 *M*) and cysteine (0·01 *M*). 200 mgm. γ-globulin (series *A*) or 8 mgm. specifically purified antibody (series *B*) present in each mixture. Total volume 10 ml. in series *A*, 1·4 ml. in *B*. Papain added as an aqueous solution in two equal portions, at zero time and after 1½ hr.

† Sedimentation at 59,780 r.p.m. in 'Spinco' model *E* ultracentrifuge. Concentration of protein in sedimentation of series *A*, 10 mgm./ml. except for sample 1*A* (15 mgm./ml.); concentration in series *B*, 5·8 mgm./ml.

‡ Equilibrium dialysis in triplicate at 5·0 ± 0·1° C. with mean deviations shown. Free concentration of radioactive *p*-iodobenzoate after equilibration; in series *A*, $2{\cdot}74 \times 10^{-4}$ *M*; in series *B*, $4{\cdot}49 \times 10^{-4}$ *M*. Maximum deviation in free concentration in either series, 3 per cent.

In contrast with the large decrease in molecular size are the small changes in binding capacity. For samples 2*B* or 4*A*, which exhibited a single peak at 3·5 or 3·6 S., the concentration of hapten bound was more than 90 per cent as large as that for the corresponding untreated antibody preparation at the same free hapten concentration. Since the free hapten concentration was held constant, the results indicate that the decrease in the average binding constant or in the total number of binding sites was less than 10 per cent[6]. The use of larger amounts of papain resulted in somewhat greater decreases in binding capacity (up to 25 per cent) but most of the combining sites were evidently still intact.

Treatment with papain resulted in each case in complete loss of ability to precipitate with the ovalbumin–*p*-azobenzoate test antigen, as shown by tests made with a wide range of antigen concentrations. Control experiments, in which the papain was inactivated by treatment with 0·01 *M* ferricyanide prior to addition of antigen, showed that this failure to precipitate was due to the action of papain on the antibody (rather than on the antigen).

The results indicate that the rabbit antibody molecule can be degraded to a non-precipitating form of relatively low molecular weight with little effect on the specific combining sites. It appears also that measurements of binding capacity provide a convenient quantitative method for investigating the properties of these apparently univalent antibody fragments.

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Regeneration of Cellulose Acetate Membranes used for Zone Electrophoresis

The method described by Kohn[1] for the analysis of serum proteins by zone electrophoresis on strips of cellulose acetate membrane has been proved to be of great value. The development of this technique, as a micro-electrophoretic method[2], has emphasized the importance of cellulose acetate as a stabilizing medium. The sheets of cellulose acetate are relatively expensive compared with filter paper, and their higher cost may deter workers from using this excellent material. It has been found that the cellulose acetate membrane strip may be used several times after suitable treatment by the method to be described.

On completion of an analysis the used strip of cellulose acetate membrane with the stained bands of protein is placed in a shallow dish containing 200 ml. of buffer at *p*H 8·6 (veronal, sodium bicarbonate or sodium borate, ionic strength 0·05–0·1) to which is added 2·0–4·0 ml. trypsin solution. The trypsin solution contains 64 units of trypsin per ml. and is standardized by the method described by Coles[3]. The digestion of the protein occurs rapidly for quantities of protein in the range 100–200 μgm. and is complete at 37–39° C. in 2 hr.; but the digestion of 700 μgm. of protein, used with longer and wider strips, is not complete in 2 hr. and sometimes required periods of 16 hr. The longer digestion period is necessary to regenerate strips of cellulose acetate membrane which have been stored in a dry condition for some months after staining. After digestion of the protein, the cellulose acetate membrane strip is washed with distilled water and re-equilibrated in the buffer solution used for the electrophoretic separation. It is then ready for further use.

The cellulose acetate is quite stable under the conditions of regeneration: and in one instance a membrane strip has been regenerated four times without any apparent deterioration; this strip was dyed with lysamine green, bromphenol blue, nigrosine and finally bromphenol blue again for its use on the first, second, third and fourth occasions respectively.

In one instance 700 μgm. of serum protein had been separated into its component fractions by electrophoresis on a cellulose acetate membrane in a buffer solution to which copper sulphate had been added. The regeneration process, performed several months after the membrane had been dried, was not complete. The globulin fractions were digested by the trypsin; but the large stain corresponding to the albumin fraction was not removed. In this case, after treatment with trypsin, the membrane was washed with distilled water, then twice with ethyl alcohol and was finally equilibrated against benzene. The membrane appeared translucent and all traces of dye disappeared. The membrane was then washed with ethyl alcohol, followed by distilled water and finally re-equilibrated against the buffer solution unchanged. Its performance in use after regeneration was quite normal, and another sample of normal serum protein was fractionated successfully.

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Amperometric Determination of Sulphydryl Content of Blood and Tissues

THE total, non-protein and protein sulphydryl contents of biological materials can be determined by using two different analytical procedures: one for the determination of total sulphydryl (protein sulphydryl + non-protein sulphydryl) values and the other for the determination of non-protein sulphydryl values. The protein sulphydryl values are obtained by subtracting the non-protein sulphydryl values from the protein sulphydryl values. Most of the available methods, however, are often complicated, time-consuming and more or less non-specific. In an earlier communication[1] it was reported from this laboratory that, with a little modification, the amperometric method of Benesch, Lardy and Benesch[2] can be used for the specific determination of the total sulphydryl content of biological materials. It has since been observed that the method can be adapted for the non-protein sulphydryl determinations in the same materials. Thus it is possible to use a simple, specific and rapid method for the determination of all the three sulphydryl fractions in biological materials.

Table 1. CONCENTRATION OF TOTAL, NON-PROTEIN AND PROTEIN SULPHYDRYL IN RAT BLOOD AND TISSUES

The means of results with number of determinations in parentheses are given. The results are expressed in mgm./100 gm. for wet tissues and mgm./100 ml. for blood

Sample analysed	Total sulphydryl	Non-protein sulphydryl	Protein sulphydryl*
Whole blood	91·4 (8) 83·6–100·0	2·9 (4) 2·5–3·3	88·5
Liver	55·4 (7) 51·4–58·8	26·4 (4) 23·4–32·0	29·0
Kidney	48·3 (3) 45·0–53·0	11·3 (4) 8·7–13·1	37·0
Heart	31·2 (3) 28·0–33·0	7·2 (4) 6·8–7·9	24·0
Pancreas	25·4 (6) 20·0–30·4	6·7 (4) 6·5–6·9	18·7

* These values were obtained by subtracting the means of non-protein sulphydryl values from the means of total sulphydryl values.

The amperometric titration apparatus, with the exception of the platinum electrode, is assembled according to Benesch, Lardy and Benesch. For the construction of the platinum electrode, four strands (each 35 mm. long and 0·5 mm. diam.) of tightly twisted platinum wires are sealed in a soft-glass tubing in such a way that 15–20 mm. of the bare metal protrudes beyond the glass. The projecting wires are bent laterally into a U-shape. This electrode, in contrast to that used by others[2], greatly increases the sensitivity of the apparatus. For total sulphydryl determinations tissue homogenates and hæmolysed blood samples[1] are used. For non-protein sulphydryl determinations, sulphosalicylic acid (2·5 per cent solution) extracts of blood and tissues prepared according to the method of Benesch and Benesch[3] are used. Tissue homogenates (1–2 ml.), whole blood (0·1 ml. hæmolysed with 1 ml. of de-ionized distilled water), and sulphosalicylic acid extracts (1–5 ml.) of blood and tissues are titrated with 0·002 *M* silver nitrate in *tris* (hydroxymethyl) aminomethane buffer[2], *p*H 7·4. Each increment (0·1–0·2 ml.) of silver nitrate is added at 1-min. intervals and the microammeter reading is taken at 55 sec. after each addition. It is essential to neutralize the sulphosalicylic acid extracts before addition to the buffer. The neutralization is carried out with 0·8 *M* sodium bicarbonate, the amount being determined in a separate titration using methyl orange as an indicator. During the titration of tissue homogenates pieces of connective tissue may adhere to the platinum electrode and cause mechanical interference with current readings. This is easily eliminated by first rotating the electrode for a few minutes in the buffer containing the homogenate (before the addition of silver nitrate) and then removing the adhering pieces from the electrode. To increase the specificity of the method a 'blank' titration is carried out with each estimation, using an excess of a 0·001 *M* solution of *p*-chloromercuribenzoate in 0·05 *N* sodium hydroxide. The technique of the 'blank' titration is very similar to those used by other workers[4,5]. Where x ml. of 0·002 *M* silver nitrate is required for a titration, $2x + 0{\cdot}1$ ml. of 0·001 *M* *p*-chloromercuribenzoate solution is added to block the sulphydryl groups. *p*-Chloromercuribenzoate is allowed to react for 5 min. before the titration. The final volume of the titration mixture, including the *p*-chloromercuribenzoate solution, is kept at 30 ml. Calculation: 1 ml. of 0·002 *M* silver nitrate $\equiv$ 0·066 mgm. of sulphydryl.

The results of sulphydryl determinations, using the above method, in blood and tissues of rats (200–350 gm., both male and female) are summarized in Table 1.

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Selenite as a Relatively Weak Inhibitor of some Sulphydryl Enzymes

CONTINUING our studies on the oxidation of sulphydryl compounds[1], we found that selenite was a very active catalyst for the oxidation of cysteine, glutathione, dihydrolipoic acid, and coenzyme *A*. For the oxidation of glutathione, selenite was a far better catalyst than cupric chloride, the most active catalyst known previously[2]. Selenite is poisonous to animals but it is also claimed that selenium is an essential element. Selenium poisoning is an important unsolved problem: the toxic symptoms are complex and the mechanism of the poisoning is still obscure. Some investigators[3,4] believe that selenium compounds exert their toxic effect through interference with certain enzyme systems in the living organism, particularly through mercaptide formation with sulphydryl enzymes. However, most studies on enzyme inactivation had been done with tissue homogenates or yeast cells; and further information on the reaction of selenium salts with purified enzymes seemed desirable. The present communication reports the results of a study of the comparative inhibition of six well-known sulphydryl enzymes by selenite, cupric chloride, and *p*-chloromercuribenzoate.

Table 1. Inhibition of Sulphydryl Enzymes

Inhibitor	Concentration (*M*)	Inhibition (per cent)					
		Alcohol dehydrogenase	Aldolase	D-amino-acid oxidase	β-Amylase	Lactic dehydrogenase	Urease
Selenite	0·01	48	29	—	—	—	—
	0·001	3	1	60	62	0	49
	0·0001	0	0	54	40	0	6
Cupric chloride	0·001	100	100	100	91	100	89
	0·0001	100	98	44	57	99	10
	0·00001	75	2	—	—	0	—
p-Chloromercuribenzoate	0·001	100	100	100	100	100	100
	0·0001	100	50	66	91	100	96

Yeast alcohol dehydrogenase (2× crystallized), muscle aldolase (5× crystallized), and kidney D-amino-acid oxidase were purchased from the Worthington Biochemical Corp.; muscle lactic dehydrogenase (crystallized) and urease (type *V*, special purity) from Sigma Chemical Co.; and barley β-amylase from Nutritional Biochemicals Corp.

The activities of alcohol dehydrogenase, aldolase, D-amino-acid oxidase, lactic dehydrogenase, urease, and β-amylase were measured by the methods of Racker[5], Swenson and Boyer[6], Frisell and Hellerman[7], Mehler *et al.*[8], Krebs and Henseleit[9], and Bernfeld[10], respectively. For the inhibition tests, the enzyme was incubated at room temperature with the inhibitor for at least 30 min. before determination of the activity.

The results in Table 1 show that, as compared with cupric chloride and *p*-chloromercuribenzoate, selenite is a rather weak inhibitor for the enzymes studied.

Lethal doses of selenite for several mammalian species are in the range of 1–15 mgm./kgm. body-weight[3]. In view of the fact that selenite is toxic in such small quantities, it does not seem possible that the effect is the result of mercaptide formation with sulphydryl compounds, such as glutathione and sulphydryl enzymes, *in vivo*. Also, from the results of our present studies, it appears unlikely that selenium toxicity is a result of direct inhibition of enzymes. It is more likely that the toxicity may be explained by the selenium-catalysed oxidation of such co-factors as glutathione, coenzyme *A*, and dihydrolipoic acid, and the resulting disturbances in intermediary metabolism.

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Labelled Keto-Acids in Duck Blood incubated *in vitro* with $NaH^{14}CO_3$

The ability of duck blood rich in reticulocytes to incorporate *in vitro* carbon-14 from $NaH^{14}CO_3$ has been previously investigated[1]. Separately we have demonstrated incorporation into globin, hæm and stroma. The widespread incorporation suggested that the first products of carboxylation belonged to a pool from which all the substances we examined would be synthesized. This communication is concerned with an attempt to identify some labelled precursors (pyruvic and α-ketoglutaric acids).

45 ml. of duck blood were incubated for 18 hr. as previously described[1]. The plasma was separated from red cells by centrifuging. The plasma proteins were precipitated with 5 per cent metaphosphoric acid (4 ml. for 1 ml. of plasma) and discarded. The keto-acids were isolated from the supernatant as 2 : 4-dinitrophenylhydrazones[2], which were separated by paper chromatography (Whatman No. 1; *iso*-butanol, ammonia, 95 per cent ethyl alcohol, 70 : 20 : 10, v/v/v).

The 2 : 4-dinitrophenylhydrazones of pyruvic and α-ketoglutaric acids and an unidentified substance (R_F, 0) were eluted from the paper with 2 *N* sodium hydroxide. The eluate was acidified with hydrochloric acid and extracted with ethyl ether. After evaporation of the ether, the substances extracted were assayed for radioactivity by the method previously described[2].

In Table 1 the R_F of 2 : 4-phenylhydrazones of pyruvic and α-ketoglutaric acids and the specific activities of pyruvic and α-ketoglutaric acids are shown. The specific activity of the unidentified substance was 7,500 disintegrations/min./mgm. carbon. No attempt has yet been made to identify it.

Table 1

Material	Pyruvic acid	α-Ketoglutaric acid
Specific activity (dis./min./mgm. C)	1,820 ± 40	1,790 ± 50
R_F of 2 : 4-dinitrophenylhydrazone	0·38 (isomer 1) 0·58 (isomer 2)	0·13

The presence of labelled α-ketoglutaric acid seems to suggest that the Krebs cycle is effective in carbon dioxide fixation by duck blood *in vitro*. The labelled pyruvic acid also seems to be formed from the Krebs cycle acids (that is, oxalacetic acid). Moreover, labelled pyruvic acid could be formed via another pathway:

ribulose-5-phosphate $\xrightarrow{CO_2}$ 6-phosphogluconate
6-phosphogluconate ⟶ glucose-6-phosphate
glucose-6-phosphate ⟶ 2 pyruvic acid

The incorporation of carbon dioxide by certain normal and tumour cells (but not by rabbit erythrocytes) in ribulose-5-phosphate was demonstrated by Baron *et al.*[4].

As succinic acid is formed via α-ketoglutaric acid in the Krebs cycle, and because succinic acid is a precursor of hæm[5], the hæm should also be labelled after incubation *in vitro* of duck blood with $NaH^{14}CO_3$.

Experiments in progress suggest that only part of the carbon-14 activity found in globin is due to amino-acids formed by transamination from α-ketoglutaric, oxalacetic and pyruvic acids. Indeed, arginine is also labelled.

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Purification of a Toxic Phytohæmagglutinin from Black Beans (*Phaseolus vulgaris*)

Beans, like other seeds of the Leguminosae, contain some proteins of remarkable biological activity. For many years a bean phytohæmagglutinin has been known[1,2]. Rigas and Osgood[2] recently described two different factors with hæmagglutinating properties. Other authors[4,5] have observed toxic effects in animals fed with raw beans or with certain preparations obtained by extracting bean meal. The hæmagglutinin factors prepared according to the original method[2] do not seem to be responsible for the toxic effect of raw beans, as they have only slightly toxic properties. Experiments carried out by one of us (W. J.) have also established the toxicity of beans that cannot be ascribed to the trypsin inhibitor (another protein) described by Bowman[6]. We therefore undertook to isolate the toxic components of beans in order to obtain further information about their properties.

Finely ground black beans (*Phaseolus vulgaris*) are extracted with 5 parts (w/v) of a solution of 1 per cent sodium chloride. By dialysing the filtered extract against water a large amount of inactive material is precipitated. Almost the whole activity is obtained by saturating the supernatant solution with ammonium sulphate. Different active fractions are prepared by saturating fractionally the redissolved precipitate with the same salt.

The main active fraction is obtained by precipitation between 55 and 70 per cent saturation of the redissolved, dialysed crude precipitate which has been previously adjusted to *p*H 6·0. After the precipitating and redissolving procedures have been repeated three times a highly purified protein is obtained which proves to be at least 90 per cent pure as revealed by electrophoresis and ultracentrifugation. The electrophoretic mobility, determined with the 'Spinco' apparatus model *H*, is $8{\cdot}89 \times 10^{-5}$ cm.²/volt/sec. for a 0·9 per cent solution in barbiturate buffer of *p*H 8·6 and ionic strength 0·1. The isoelectric point should lie between *p*H 4 and 5. The sedimentation constant S_{20} for a 0·6 per cent solution in the same buffer was $5{\cdot}9 \times 10^{-13}$ ('Spinco' ultracentrifuge model *E*). The protein, for which we propose the name 'phaseolotoxin *A*', is soluble in water, in 1 per cent sodium chloride solution and is precipitated by 30 per cent cold alcohol. It contains 13·5 per cent nitrogen (Kjeldahl) and 5·0 per cent reducing groups as determined by a modified Somogyi–Nelson method and calculated for glucose. These values relate to air-dried preparations.

Table 1. Physical and Biological Properties of Bean Fractions

Fraction	Electrophoretical mobility (cm.²/V./sec. × 10^5)	Sedimentation constant ($S_{20} \times 10^{13}$)	Agglutinating activity (*c*) (μgm./ml.)	Toxic activity: Dose (mgm./kgm.)	Toxic activity: Mortality (*d*)
Phaseolotoxin *A*	8·89 (*a*)	5·9 (*a*)	3·0	50	15/2
				60	6/5
				100	9/9
Phaseolotoxin *A* after treatment by stroma	—	—	∞	100	4/0
Phaseolotoxin *A* heated to 80° C.	—	—	6·0	125	7/3
Fraction *B*	24·5 (*a*)	2·3 (*a*)			
	28·5 (*a*)	6·6 (*a*)	4·0	150	5/2
		9·7 (*a*)		200	5/3
	1·04 (*a*)	2·3 (*b*)			
	4·35 (*b*)	6·5 (*b*)			
	5·79 (*b*)	10·6 (*b*)			
		18·0 (*b*)			

(*a*) Barbiturate buffer, *p*H 8·6; μ 0·1. (*b*) Phosphate buffer, *p*H 6·5; μ 0·1. (*c*) Minimum amount of toxin which gives positive results. (*d*) Toxin in mgm./kgm. mouse: number of animals used for the experiment/number of animals which died after injection.

Saturating the supernatant solution obtained after separating phaseolotoxin *A* by filtration yields another active fraction. The yield of this so-called 'fraction *B*' is low and not uniform. Even when precipitated three times it consists of three main components, as revealed by electrophoretic and centrifugation experiments. Among these no compound with the physical properties of phaseolotoxin *A* could be found.

The hæmagglutinating activity was determined with 0·25 per cent suspensions of washed sheep or mice erythrocytes in saline. 1·0 ml. of this suspension is mixed with 1·0 ml. of the active preparations obtained from beans in adequate dilutions and incubated at 37° C. for 30 min. and then centrifuged at 2,500 r.p.m. in a common laboratory centrifuge. The agglutination test is positive when the erythrocytes are not suspended by shaking.

Toxicity was studied in adult white mice after intraperitoneal injection of adequate dilutions of the preparations. The results of these experiments are shown in Table 1.

There are striking differences between phaseolotoxin *A* and the fraction *B*. These fractions have about the same agglutinating activity, but fraction *B* is at least three times less toxic. Therefore it may be assumed that black beans contain more than one toxic compound. Fraction *B*, different from phaseolotoxin *A* in its physical properties, also shows biological differences, and because of its relatively low toxicity and physical properties it resembles more the phytohæmagglutinin described by Rigas and Osgood[2].

Phaseolotoxin *A* proved to be a single component as revealed by the physical investigations, and these results suggest that hæmagglutinating and toxic properties are due to the same protein. This suggestion

is strengthened by two other experiments. When a solution of 50 mgm. of the toxin is incubated for 30 min. at 37° C. with a suspension of 100 mgm. of human erythrocyte stroma in saline[7] the toxin is completely absorbed and after centrifugation the supernatant contains neither agglutinating nor toxic activity. Moreover, when a saline solution of phaseolotoxin *A* in a concentration of 25 mgm./ml. is heated to 80° C. for 60 min. the activities are not completely destroyed, although the solution becomes milkily turbid due to partial denaturation and precipitation. Hæmagglutinating and toxic activity, however, are reduced to the same extent, namely, to about 50 per cent. Boiling for 30 min. uniformly abolishes both activities completely.

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CELL PHYSIOLOGY

Nuclear Origin of Ribonucleic Acid

Living hyphæ of *Neurospora crassa* were centrifuged and their contents stratified in distinct layers. Starting at the centrifugal end, the layers were as follows (Fig. 1): glycogen, ergastoplasm (microsomes), mitochondria, nuclei, 'supernatant' cytoplasm, vacuoles and fat. Each fraction could be identified by cytochemical reactions. It was found that most of the cytoplasmic ribonucleic acid resided in ergastoplasm, some in mitochondria, and none was detectable in the 'supernatant'. Nuclei were relatively poor in ribonucleic acid.

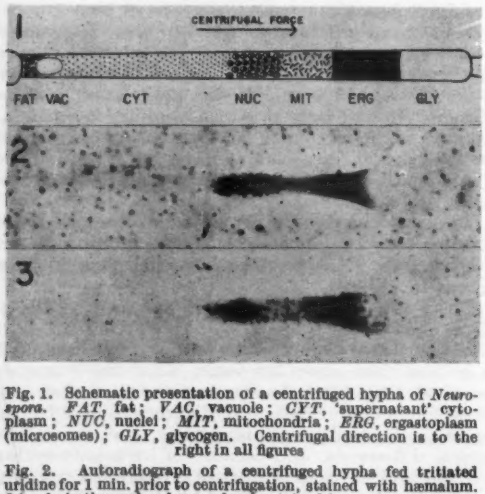

Fig. 1. Schematic presentation of a centrifuged hypha of *Neurospora*. *FAT*, fat; *VAC*, vacuole; *CYT*, 'supernatant' cytoplasm; *NUC*, nuclei; *MIT*, mitochondria; *ERG*, ergastoplasm (microsomes); *GLY*, glycogen. Centrifugal direction is to the right in all figures

Fig. 2. Autoradiograph of a centrifuged hypha fed tritiated uridine for 1 min. prior to centrifugation, stained with hæmalum. β-tracks in the nuclear layer; deeply stained layer, free of tracks, is ergastoplasm; glycogen and 'supernatant' not stained

Fig. 3. Autoradiograph of a centrifuged hypha fed tritiated uridine for 1 min. and unlabelled uridine for 1 hr., prior to centrifugation. β-tracks densest in ergastoplasm, less dense in nuclei and mitochondria; no significant increase of tracks over background in glycogen and 'supernatant'

The fact that nuclei became clearly separated from the cytoplasmic ribonucleic acid enabled us to localize the site of formation of ribonucleic acid. Mycelium, in its active growth phase, was fed tritiated uridine (uridine-5,6-^{3}H, 640 mc. per m.mole, 100 μgm./ml.) for given times, centrifuged and fixed. The mycelium was then washed in cold 5 per cent trichloracetic acid so that only nucleosides which were incorporated into ribonucleic acid remained. The preparation was mounted on a slide and covered with photographic emulsion[1]. After exposure and development, the autoradiographs showed tracks of β-decay at the sites of uptake of the precursor.

At short feeding times, from 1 to 4 min., the label appeared only in the nuclear fraction (Fig. 2). After 4 min., more and more label was found in cytoplasmic ribonucleic acid. In 1 hr., ergastoplasm was labelled much more heavily than the nuclei (Fig. 3), and some activity was found also in mitochondria, while the 'supernatant' remained virtually inactive. In brief, the distribution of the label became roughly proportional to the relative content of ribonucleic acid in the different fractions. When tritiated uridine was fed for 1 min. only and then washed out and replaced with an excess of non-labelled uridine, the label again appeared first in nuclei, to appear in the ergastoplasm only after several minutes. The labelling of ergastoplasm increased with time at the expense of nuclear label.

These results demonstrate that all the cellular ribonucleic acid is formed in nuclei and that it migrates into the cytoplasm later. Previous work of several investigators indicated that ribonucleic acid originated in nuclei[2] and the present experiments substantiate this hypothesis. These findings suggest that ribonucleic acid is a direct product of gene action. Ribonucleic acid is formed in nuclei, the seat of chromosomes and genes; it migrates into the cytoplasm; and it is required for the synthesis of proteins. A detailed report of this investigation will be published elsewhere.

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Hæmoglobin and 'Apparent Protein' in the Human Red Cell

The concentration C_{Hb} of hæmoglobin is conventionally measured by dividing the hæmoglobin in gm. per cent by the fractional concentration ρ, allowance being made for incomplete packing in the hæmatocrit tube. If a high-speed hæmatocrit is used, ρ can be found with an error of about ± 1 per cent, and the hæmoglobin concentration can be measured by the cyanmethæmoglobin method with an error of about the same magnitude.

An independent method measures the concentration C_i in gm. per cent of what can be called 'apparent protein'; this includes hæmoglobin, colourless hæmoglobin precursors, lipoproteins, and 'stromatin'. Other substances contribute to a very small extent. The measurements of C_i are made with an *AO*-Baker interference microscope and with monochromatic green light, the red cells being turned into spheres by being suspended in a non-hæmolytic lecithin (Polanyi, personal communication). The equations are (Barer[1])

$$\mu = \frac{\theta\lambda}{360\,t} + n$$

and

$$C = \frac{\mu - n}{\alpha}$$

where θ is the phase retardation in degrees, λ the wave-length in the same dimensions as t, the cell thickness (equal to the diameter) of the spherical red cell, n the refractive index of the medium surrounding the cell, and α a constant called the specific refractive increment. Because the concentration of hæmoglobin in the red cell is so high, α has to be determined experimentally with a calibrated Nichols refractometer[2]. The calibration can be made with water ($n = 1{\cdot}333$), acetone ($n = 1{\cdot}3592$), and *n*-butyl alcohol ($n = 1{\cdot}3991$), all at 20° C.

About 1 ml. of normal or hypochromic blood is packed at high centrifugal force ($2 \times 10^4 g$), the supernatant column of plasma is removed, and the cells in the red cell column are hæmolysed by freezing and thawing several times; the hæmolysed sample is then spun in a high-speed centrifuge in order to free it as much as possible from ghosts, etc. The almost ghost-free supernatant column is removed, and its hæmoglobin concentration is found by the cyanmethæmoglobin method. Its refractive index is found with the Nichols refractometer by reference to the calibration curve. A graph is prepared in which refractive index is plotted against hæmoglobin concentration, straight lines being drawn for refractive index increments of 0·0020, 0·0018, etc. A vertical line is drawn through the determined hæmoglobin concentration, and a horizontal line is drawn through the determined refractive index. These two lines intersect at a point which corresponds to a value of the refractive index increment α. If the same experiment is repeated by determining the refractive index of the sample diluted with water in powers of 2, it is clear that the specific refractive increment is virtually constant with increasing hæmoglobin concentration, being essentially the same ($\alpha = 0{\cdot}0019 \pm 0{\cdot}0002$) when the hæmoglobin is dilute as when it is as concentrated as 30 per cent. It is a pleasure to thank Dr. H. G. Davies for telling me that this value of α is correct in his experience.

The mean value of C_{Hb} in normal males is $30{\cdot}1 \pm 0{\cdot}32$ gm. per cent. The value of C_i found by the refractometer is 33·28 gm. per cent in normal males, that is, about 10 per cent greater if C_{Hb} is considered as 100. It is inconceivable that this amount of 'apparent protein, not hæmoglobin' could be held in a surface ultrastructure of a thickness such as has been proposed, for, if it were, the ultrastructure, *dry*, would have to be about 1000 A. thick. No such value has been suggested, and so at least two-thirds of the 'apparent protein, not hæmoglobin' would have to be distributed in the red cell interior. Nothing can be said as to how it is distributed, except that if it were distributed uniformly, its concentration would be about 7 per cent of the hæmoglobin concentration.

In the case of persons with a hæmoglobin concentration of about 25 gm. per cent, the average discrepancy between C_i and C_{Hb} is about 16 per cent, C_{Hb} being again put at 100 gm. per cent. Over the range $C_{Hb} = 25$–35 gm. per cent, the refractive index increment α is substantially constant.

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ANIMAL PHYSIOLOGY

Detection of Heterozygous Carriers in Glycogen Storage Disease of the Liver (von Gierke's Disease)

IN 1929, von Gierke[1] described a rare congenital disorder of carbohydrate metabolism characterized by excessive deposition of glycogen in various organs of the body. Cori and Cori[2] have shown that there is a deficiency of glucose-6-phosphatase activity in the liver in such patients. The occurrence of this condition in siblings[3] and among the offsprings of consanguineous matings[4], together with its equal distribution in both sexes, suggests that it is probably transmitted by a single autosomal recessive gene. In this situation, one would expect the disease to occur only in persons who receive two of the abnormal genes, one from each parent. If an individual is heterozygous for the condition, he will usually be free of clinical symptoms. Careful biochemical studies, however, might reveal minor departures from the norm[5].

Kapila, Kaul and Chattesjee[6] have reported on a family where the father, mother and siblings of a patient with von Gierke's disease showed a borderline response of blood glucose-levels to 0·25 ml. of adrenalin intramuscularly. One of the siblings also showed a flat oral glucose tolerance curve.

Recently, Wagner, Meyerriecks and Sparaco[7] have shown that there is an excessive accumulation of phosphorylated intermediates of the glycolytic cycle in the whole blood and plasma of patients with von Gierke's disease. Similar changes have now been noted among the parents of such patients and forms the basis of the present communication.

Glucose-6-phosphate and fructose-6-phosphate were determined in the following manner: 3–4 ml. of whole blood was collected in heparin and the serum was removed by centrifugation. The red cells were washed with normal saline, weighed and dissolved in 10 per cent trichloracetic acid. It was found that between 0·3 and 0·5 gm. of red cells dissolved in 8 ml. of trichloracetic acid gave the best results.

A 'Dowex-1' (10 × 100–200 mesh) column, which had previously been washed first with 20 per cent formic acid until free of chloride and afterwards with distilled water, was used for separation. All the trichloracetic acid extract was placed on the column and washed with 10 ml. of distilled water. The glucose-6-phosphate was eluted with 0·2 *M* ammonium formate solution *p*H 7·25. The fructose-6-phosphate was eluted with 0·5 *M* ammonium formate *p*H 7·25. The eluant was collected in separate 25-ml. volumetric flasks and collection was continued until three successive flasks were free of sugar. The colour

Table 1. RESULTS OF DETERMINATIONS DONE ON NORMAL CONTROLS, HETEROZYGOTES AND AFFECTED INDIVIDUALS EXPRESSED AS GM./100 GM. RED CELLS

Statistical analysis for hererozygotes versus controls of glucose-6 phosphate (G-6-P) showed $t = 5{\cdot}33$ and fructose-6-phosphate (F-6-P) showed $t = 9{\cdot}01$.

	Normals		Heterozygotes		Affected	
	G-6-P	F-6-P	G-6-P	F-6-P	G-6-P	F-6-P
No.	10	10	8	8	4	4
Mean	0·18	0·38	1·10	1·56	1·09	1·30
S.D.	±0·12	±0·14	±0·45	±0·32	±0·34	±0·20
Range	0·05–0·37	0·12–0·57	0·70–2·17	0·86–2·07	0·80–1·67	1·15–1·62

was developed with anthrone reagent and read at 620 mμ in the Beckman *DU* spectrophotometer.

The results were compared with those obtained from standard solutions of glucose-6-phosphate and fructose-6-phosphate and expressed as gm./100 gm. of red cells.

The results from 10 normal controls, 8 heterozygotes (parents of affected children) and 4 patients with von Gierke's disease are given in Table 1. It is apparent that the levels of glucose-6-phosphate and fructose-6-phosphate in the red cells of heterozygotes are significantly different from those of the normal controls and more closely resemble that of those with the disease.

These findings are in agreement with the data of Wagner, Meyerriecks, and Sparaco[7], who demonstrated the accumulation of phosphorylated intermediates of the glycolytic cycle in the peripheral blood of patients with von Gierke's disease. While the results in a single individual must be interpreted with caution, the abnormal values for glucose-6-phosphate and fructose-6-phosphate in a group of known heterozygotes suggest that this may be a phenotypic expression of a single recessive gene for von Gierke's disease. This would provide additional confirmation for the view that the lack of glucose-6-phosphatase is the primary lesion in the disease and extend the concept of being able to detect heterozygous carriers biochemically, as has been shown in the abnormal hæmoglobins[8], phenylketonuria[9] and galactosæmia[10].

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An Enzymatic Basis for the Acceleration of Ciliary Activity by Negative Air Ions

IT has recently been shown that small negative air ions accelerate the tracheal ciliary activity of mammals both *in vitro* and *in vivo*[1,2]. The subsequent discovery that the actual mediator of these effects is negatively ionized oxygen[3] suggested the possibility of a direct action by negative air ions on one or more of the respiratory enzymes. This seemed increasingly likely when it was found that: (*a*) dilute solutions of sodium azide (as low as 1×10^{-5} molar) inhibit ciliary activity; (*b*) the hæmoglobin of animals killed with carbon dioxide recovers its bright red colour more rapidly in a negatively ionized atmosphere than in ordinary air.

Accordingly, a modified Keilin–Hartree pig heart homogenate[4,5] was prepared and the effect of (−) air ions on its catalytic activity was studied.

The rate of conversion of succinate to fumarate by the homogenate was followed by observing the increase in optical density of the mixture at 250 mμ[6]. Test solutions were exposed in a variety of ways with substantially the same results; in the most convenient method, 0·9 ml. 1 molar disodium succinate, 2·0 ml. boiled and cooled 0·2 molar phosphate buffer at *p*H 7·3, and 0·1 ml. heart homogenate (1 : 10 dilution) were combined in a Beckman silica glass cuvette. After the zero-time reading, the cuvettes were placed in specially designed chambers which permitted exposure to air ions while the solutions were being stirred with magnetic stirrers. Unipolar ion densities of 1×10^8/cm.^{3}/sec. at the fluid–air interface were delivered by 'Wesix' ionizers[7,8] consisting of tritium in a sealed foil with a reversible rectifying circuit to permit selection of either (+) or (−) air ions. At regular intervals the cuvettes were removed and their optical densities read in a Beckman *DU* spectrophotometer. Experiments were run at room temperature; periodic readings with an electronic

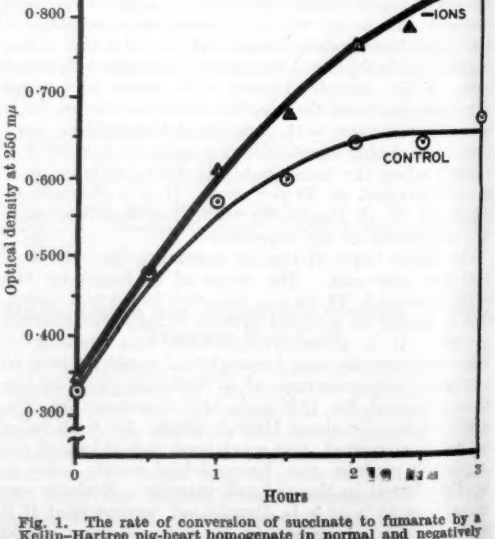

Fig. 1. The rate of conversion of succinate to fumarate by a Keilin–Hartree pig-heart homogenate in normal and negatively ionized atmospheres

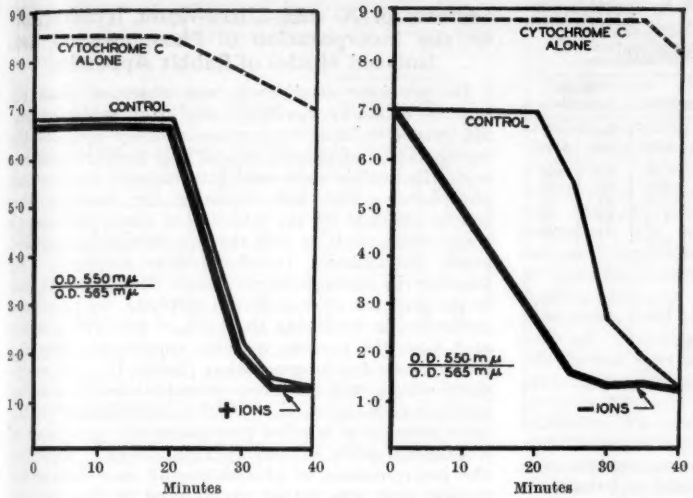

Fig. 2. The re-oxidation by cytochrome oxidase in normal and ionized atmospheres of cytochrome *c* reduced by sodium hydrosulphite. The dotted line shows the behaviour of cytochrome *c* in the absence of cytochrome oxidase in ionized and un-ionized atmospheres

thermometer established that there was no temperature variation between test and control samples.

As shown in Fig. 1, fumarate formation was more rapid in the negatively ionized sample. Positive ions were without effect.

In order to localize further the action of negative ions, experiments were performed on the rate of oxidation of cytochrome *c* alone and on cytochrome *c* plus heart homogenate. A 50-micromolar solution of commercial horse-heart cytochrome *c* ('Sigma') in 0·2 molar phosphate buffer at *p*H 7·3 was reduced with sodium hydrosulphite; 3 ml. samples were placed in control and ionizing chambers; re-oxidation was followed by regular spectrophotometric readings at 550 mμ and 565 mμ according to Paul's method[9]. Positive and negative air ions were without effect on the re-oxidation of cytochrome *c* alone. However, when 0·1 ml. of the cytochrome oxidase-containing Keilin–Hartree homogenate (1 : 10 dilution) was added to the solution, negative ions had a distinctly accelerating effect on the re-oxidation of cytochrome *c*; positive ions had no effect (Fig. 2).

Since sodium hydrosulphite may react with air to form hydrogen peroxide, experiments were performed in which the cytochrome *c* was reduced by either cysteine or ascorbic acid. The results were similar (Table 1). Preliminary exposure of the reductant alone (in solution) to (+) or (−) ions in no way altered the efficacy of the reductant.

It thus appears that negative air ions act directly on cytochrome oxidase, or else form a radical upon contact with water which acts directly on cytochrome oxidase. These experiments provide a reasonable explanation for the accelerating action of negative air ions on the tracheal ciliary rate. They fail to explain, however, the markedly inhibiting effects of positive air ions on the ciliary rate[1,2].

Taken in conjunction with the observed action of negative air ions on hæmoglobin, these experiments support the postulate that: it is the distinctive ability of negative air ions—or, more precisely, negatively ionized-oxygen—to react more rapidly with certain iron porphyrin compounds that is responsible for many of their physiological effects (that is, the decrease in respiratory-rate, the increase in the rate of tracheal ciliary beating, the increase in the rate of mucus flow).

We should like to thank Dr. J. B. Neilands for his helpful advice during the early phases of this work.

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The opinions advanced on this report must not be construed as representing the official views of the U.S. Naval Service.

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Table 1. Re-oxidation by Cytochrome Oxidase in Normal and Negatively Ionized Atmospheres of Cytochrome *c* reduced by Other Methods

Time of exposure (min.)	Ratio Optical density at 550 mμ / Optical density at 565 mμ (Cysteine-reduced) (C)	(−)	(Ascorbic acid-reduced) (C)	(−)
0	7·7	7·7	4·5	4·8
40	6·9	6·5	3·5	3·5
60	5·5	1·5	3·0	2·6
80	1·3	1·3	2·5	2·1

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Changes in Serum Transaminase Activity following Myocardial Damage due to Lymphatic Congestion

Ligation of the paracardial lymphatic glands and of the main paracardial lymphatic trunks results in myocardial damage and pathological changes of the electrocardiogram[1]. It is now well known that myocardial damage due to coronary occlusion is accompanied by increase of serum transaminase activity[2,3]. Starting from these facts we investigated the changes in serum transaminase activity following lymphatic obstruction.

The experiments were carried out in dogs of both sexes with average weights of 10–12 kgm. In 7 dogs under chloralose anæsthesia (0·1 gm./kgm. bodyweight) and sterile conditions a thoracotomy was performed on the right side; the paracardial lymphatic glands, the right lymphatic trunk and th

Table 1

	Glutamic-oxaloacetic acid transaminase				Glutamic-pyruvic acid transaminase			
	Operation		Sham operation		Operation		Sham operation	
	Before	After	Before	After	Before	After	Before	After
1	95	293	90	106	76	73	54	106
2	71	210	55	108	48	106	40	35
3	149	310	117	146	81	118	48	95
4	144	196	93	165	36	158	96	82
5	103	296	143	108	48	75	81	93
6	95	305	—	—	67	223	—	—
7	139	324	—	—	—	—		
No. of animals	7	7	5	5	6	6	5	5
Mean	114	276	100	127	59	126	64	82
Mean changes	163		27		66		18	
Difference of changes	$\bar{x}$	136			48			
	s	49·39			49·45			
	t	4·709			1·602			
	P(%)	<0·1			>10			

thoracic duct were ligated[4,5]. Sham-operations consisted of thoracotomy in three animals, and thoracotomy plus ligation of cervical glands and the thoracic duct in two animals. Transaminase activity was determined by Umbreit's method in blood drawn daily before and after intervention[6].

Results are given in Table 1. Maximal values up to 3 days before and after intervention are shown. It is evident that glutamic-oxaloacetic acid transaminase activity in sera after intervention increased significantly compared with the controls. Changes in serum glutamic-pyruvic acid transaminase activity were not significant.

Lymphatic obstruction as described above results in the majority of cases in a varying degree of histological change in the myocardium. This is proved by the increased activity of serum glutamic-oxaloacetic acid transaminase. This rise in glutamic-oxaloacetic acid transaminase activity after myocardial damage is analogous to the rise in glutamic-pyruvic acid transaminase activity after hepatic damage[7,8]. It remains unexplained that lymphatic congestion of the liver due to ligation of the thoracic duct does not bring about changes in serum glutamic-pyruvic acid transaminase activity.

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Effects of X- and Ultra-violet Irradiation on the Incorporation of Phosphorus-32 into Isolated Nuclei of Rabbit Appendix

In previous studies[1] it was observed that the removal of deoxyribonucleic acid from rabbit appendix nuclei isolated in sucrose severely impairs the incorporation of phosphorus-32 into deoxyribonucleic acid, ribonucleic acid, and into organic acid-soluble phosphate. The lost capacity for incorporation can be restored by the addition of some polyanionic compounds, such as calf thymus deoxyribonucleate, yeast ribonucleate, or chondroitin sulphate. By treating the nuclei with pancreatic deoxyribonuclease in the presence of chondroitin sulphate, we have now succeeded in removing the bulk of deoxyribonucleic acid from the nucleus without appreciably affecting its capacity for incorporation (Table 1). An essentially similar but far more extensive observation has recently been reported by Allfrey and Mirsky[2] on the incorporation of labelled precursors into proteins and ribonucleic acids of calf thymus nuclei. However, the incorporation of phosphorus-32 into deoxyribonucleic acid was rather exceptional in this respect, because it could not be retained by chondroitin sulphate when the removal of deoxyribonucleic acid was too extensive (Table 1). This observation confirms the previous finding using sodium nucleates as the restoring agent[1].

Table 1. Effect of Treatment with Pancreatic Deoxyribonuclease in the Presence of Chondroitin Sulphate on the Capacity of Phosphorus-32 Uptake of Isolated Nuclei

Percentage of deoxyribonucleic acid removed	Total radioactivity (percentage of non-digested control)		
	Deoxyribonucleic acid	Ribonucleic acid	Organic acid-soluble phosphate
68	147	98	111
99	0	98	92

Cf. ref. 1 for the preparation of nuclei, method of incubation, and analytical procedure. The nuclei were first treated with 500 μgm./ml. of deoxyribonuclease (Worthington) in the presence of 5 mgm./ml. of chondroitin sulphate. Undigested controls were incubated in the same medium without the enzyme and chondroitin sulphate. They were then appropriately washed (ref. 1) and incubated with phosphorus-32.

We can thus have nuclei depleted of their deoxyribonucleic acid but still metabolically active. They are obviously interesting material for examining the role of deoxyribonucleic acid in the development of the irradiation lesion in the nucleus.

Isolated nuclei of rabbit appendix are much more resistant to X-irradiation *in vitro* than the lymphatic cells isolated from the same organ. Thus the X-irradiation (cf. ref. 3 for details) up to 580 r. did not affect the incorporation into deoxyribonucleic acid and organic acid-soluble phosphate; that into ribonucleic acid was not affected in some active preparations, but was inhibited by about 50 per cent in other preparations which showed relatively low incorporation into deoxyribonucleic acid (Table 2*B*). The lymphatic cells always showed a definite decrease in the uptake of phosphorus-32 into both types of nucleic acids by an X-ray dose of this order, administered *in vitro*[3].

Irradiation with ultra-violet (8–10 min. at 1 m. from a Toshiba 15-W. germicidal lamp; 16–20 times the dose killing more than 99·5 per cent of *Escherichia coli B*) reduced the incorporation into both ribo- and deoxyribo-nucleic acid by 50 per cent or more. At the lower doses (0·5–2 min.) the incorporation into ribonucleic acid was inhibited to a lesser extent while

Table 2. Effects of Depletion of Deoxyribonucleic Acid in Isolated Nuclei on the Development of Irradiation-induced Inhibition of Phosphorus-32 Incorporation into Nucleic Acids

Radiation		Concentration of deoxyribonuclease (μgm./ml.)	Percentage of deoxyribonucleic acid removed	Total radioactivity (percentage of unirradiated control)		
Type	Dose			Deoxyribonucleic acid	Ribonucleic acid	Organic acid-soluble phosphate
(*A*) Ultraviolet	10 min.	0	0	46	38	105
	8 min.	0	0	44	54	93
	10 min.	250	46	20	25	84
	10 min.	500	68	22	14	103
(*B*) X-rays	580 r.	0	0	87	41	91
		250	25	99	88	87
	540 r.	0	0	89	48	83
		500	99	—	110	67

(*A*) The nuclei were first depleted of deoxyribonucleic acid as in Table 1, irradiated, and then incubated with phosphorus-32. Because of the agglutination of nuclei, undigested controls could not be irradiated in these experiments, and are not represented here; instead, the cited values of undigested nuclei are taken from separate experiments. (*B*) The control and irradiated nuclei were incubated with or without nuclease, washed and then exposed to phosphorus-32 as in Table 1.

that into deoxyribonucleic acid was rather enhanced. Again there was no significant effect on the incorporation into organic acid-soluble phosphate (Table 2*A*). It is remarkable that in the present system the incorporation into ribonucleic acid tended to be much more sensitive to X- and ultra-violet irradiation than that into deoxyribonucleic acid, which was not inhibited at all on many occasions.

The removal of deoxyribonucleic acid (up to 68 per cent) completely failed to reduce the inhibitory effect of subsequent ultra-violet irradiation on the incorporation into the two nucleic acids (Table 2*A*). This suggests that the observed effect of ultra-violet irradiation was mediated at least in part by some entity (or entities) other than deoxyribonucleic acid. The removal of deoxyribonucleic acid (25–99 per cent) effaced the effect of X-irradiation which preceded the digestion of nuclei with deoxyribonuclease (Table 2*B*), suggesting that deoxyribonucleic acid was involved in the development of X-ray-induced inhibition of phosphorus-32 uptake into ribonucleic acid of isolated nuclei.

Unfortunately, the completion of this work had to be postponed, because the appendix nuclei become inactive during the cold seasons[1]. Details will be published later together with the results of some supplementary experiments yet to be performed.

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Inhibition of Polynucleotide Phosphorylase by Degraded Ribonucleic Acid: its Reversal by Various Agents

Studies in this Laboratory have shown that non-dialysable commercial yeast ribonucleic acid and alkaline degraded polyadenylic acid strongly inhibit the polymerization of polyadenylic acid by polynucleotide phosphorylase from *M. lysodeikticus*[1]. Substantial inhibition also occurs with ribonucleic acid from *B. subtilis* partially digested with ribonuclease. The reaction of the inhibitor with the enzyme preparation is slow and may require up to 30 min. for completion. If the substrate, adenosine diphosphate, is added simultaneously with the ribonucleic acid the rate and extent of inhibition is reduced considerably. Concentrations of yeast ribonucleic acid as low as 10^{-7} *M* (phosphate) can give up to 100 per cent inhibition of synthesis of polyadenylic acid. However, in the presence of these inhibitors the rate of polymerization is autocatalytic and shows a significant lag, the extent of which depends upon the inhibitor concentration. Both the inhibition and the autocatalytic character of the rate-curve can be reduced or abolished by the addition of acridine orange, a *B. subtilis* ribonucleic acid preparation, or polyadenylic acid (Fig. 1).

Extensive digestion of ribonucleic acid from *B. subtilis* or yeast by ribonuclease results in a considerable or complete loss of inhibitory power. Dialysable fractions have been obtained which at concentrations of the order of 10^{-7} *M* reverse the inhibition by yeast ribonucleic acid completely. This material is adsorbed quantitatively by 'Amberlite *IRC*–400(*C*1)' resin and is eluted with 0·002 *N* hydrochloric acid. Control studies with orthophosphate, pyrophosphate and the 2′- and 3′-isomers of guanylic, adenylic, cytidylic and uridylic acids show

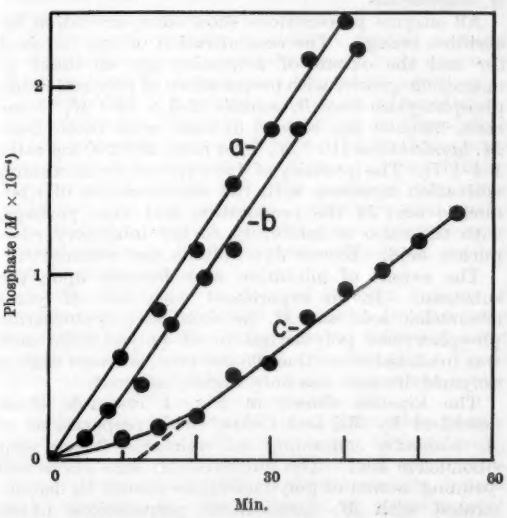

Fig. 1. Effect of yeast and *B. subtilis* ribonucleic acids on the polymerization of polynucleotide phosphorylase. Reaction mixture consisted of polynucleotide phosphorylase 113 from *M. lysodeikticus*, 0·017 *M tris*(hydroxymethylamino)methane buffer (*p*H 9·0), $1·7 \times 10^{-4}$ *M* magnesium chloride, 0·2 *M* potassium chloride, 7×10^{-4} *M* adenosine diphosphate. Mixture was incubated at 37° for 30 min. with inhibitors and/or activators before the addition of adenosine diphosphate. Curve *a*, control; curve *b*, $1·8 \times 10^{-7}$ *M* yeast ribonucleic acid, $1·8 \times 10^{-4}$ *M B. subtilis* ribonucleic acid; curve *c*, $1·8 \times 10^{-7}$ *M* yeast ribonucleic acid. Rate recorded as moles of inorganic phosphate liberated per litre of reaction mixture

no activation effect. The possibility that 2′,3′-cyclic phosphate nucleotides (cyclic-cytidylic and cyclic-uridylic acids) are involved is being investigated. Paper chromatographic studies indicate that the activity is associated with material which behaves like these compounds. Non-dialysable fractions have also been obtained which reverse inhibition by ribonucleic acid; these are probably low molecular weight or 'core' material.

The inhibition is both competitive and non-competitive with respect to adenosine diphosphate, showing a decrease in $V_{max.}$ and an increase in K_m. The inhibition is maximum at low magnesium concentrations ($< 2 \times 10^{-4}$ *M*) and decreases to a constant per cent at optimum and inhibitory concentrations of magnesium ($> 4 \times 10^{-4}$ *M*). Inhibition of ribonucleic acid increases from a negligible amount with no salt to a maximum at high salt concentration[1]. The extent of inhibition by contaminating ribonucleic acid depends upon the total acid and the ratio of inhibitory and non-inhibitory acid. Preparations of polynucleotide phosphorylase from *B. subtilis* containing high concentrations of ribonucleic acid (280/260 mμ ratio 0·5) lose activity upon standing; this activity can be recovered almost quantitatively by fractionation with ammonium sulphate or by the addition of acridine orange. Treatment of *B. subtilis* polynucleotide phosphorylase preparations with ribonuclease results in a total loss of enzyme activity, whereas similar treatment of *M. lysodeikticus* preparations shows a negligible effect. *B. subtilis* polynucleotide phosphorylase preparations partially inhibited by contaminating ribonucleic acid fail to show any salt activation, whereas freshly prepared and refractionated material is activated by potassium chloride to the same degree as are preparations from *M. lysodeikticus*[2]. Similar salt activation has been observed with preparations from *B. megaterium*.

All enzyme preparations show some activation by acridine orange. The concentration of dye required for and the extent of activation are an order of magnitude greater with preparations of polynucleotide phosphorylase from *B. subtilis* ($2{\cdot}3 \times 10^{-4}$ *M*, 75 per cent, 280/260 mμ ratio 0·5) than with those from *M. lysodeikticus* (10^{-5} *M*, 7 per cent, 280/260 mμ ratio 1·4–1·7). The quantity of dye required for maximum activation increases with the concentration of ribonucleic acid in the preparation and also, perhaps, with the ratio of inhibitory to non-inhibitory ribonucleic acid. Excess dye inhibits the reaction[3,4].

The extent of inhibition also depends upon the substrate. In an experiment with 10^{-6} *M* yeast ribonucleic acid and *M. lysodeikticus* polynucleotide phosphorylase polymerization of polyadenylic acid was inhibited more than 95 per cent, whereas that of polyuridylic acid was only slightly affected.

The kinetics shown in Fig. 1 resemble those described by Mii and Ochoa[5] with preparations of *A. vinlandii* containing an estimated 3 per cent ribonucleic acid. The autocatalytic rate curve and 'priming' action of polynucleotides cannot be demonstrated with *M. lysodeikticus* preparations unless degraded ribonucleic acid or polyadenylic acid is added. It is of interest to note, also, that salt activation observed with *M. lysodeikticus* polynucleotide phosphorylase has not been reported for *A. vinlandii* preparations, whereas the addition of degraded ribonucleic acid to *M. lysodeikticus* preparations abolishes the salt activation process. Finally, the progressive inhibition on standing of polynucleotide phosphorylase from *B. subtilis*, which can be accelerated by the addition of ribonuclease, produces an enzyme-inhibitor system which resembles the *M. lysodeikticus*–yeast–ribonucleic acid mixture and the *A. vinlandii* preparations. We suggest that the results obtained with polynucleotide phosphorylase from *A. vinlandii* by Mii and Ochoa[5] and polynucleotide phosphorylase from *M. lysodeikticus* and *B. subtilis* can be explained by a common mechanism, namely, the inhibition of the polymerization process by degraded ribonucleic acid added or present in the enzyme preparations as contaminant and the reversal of this inhibition by the addition of non-inhibitory or 'active' polynucleotides or 'primers'. A similar explanation can account for the specificity toward different substrates by various preparations of polynucleotide phosphorylase from *M. lysodeikticus* reported by Olmsted[6]. It appears probable that the specificity of priming[6] by different polynucleotides is related to the specificity of inhibition.

The mechanism of inhibition probably involves the 3′-phosphate-ended groups of the polynucleotides[1]. Judging from the initial studies with degraded ribonucleic acid, a minimum molecular weight is necessary for the inhibition. The reversal of inhibition by acridine orange is presumably the result of the specific reaction of the 3′-phosphate-ended groups with the dye to form complex II[3,4]. Reversal of inhibition by highly polymerized *B. subtilis* ribonucleic acid or polyadenylic acid suggests a competitive displacement of the 3′-phosphate-ended polymers from the active enzyme site. The partial reversal of inhibition with increasing substrate concentrations is in conformity with the earlier finding[5] that increased adenosine diphosphate concentrations increase the concentration of molecules of polyadenylic acid. The fact that there is a non-linear relationship between substrate and polymer concentrations may explain the non-competitive character of the inhibition. The autocatalytic nature of the inhibited catalysis of synthesis of polyadenylic acid reflects the increase in concentration of polymer molecules during the reaction[5], that is, the *de novo* synthesis of polymer.

The mechanism of action of the dialysable and non-dialysable fragments of ribonucleic acid is more obscure but may be related to the findings reported by Heppel[9] that 3′-phosphate-ended oligonucleotides activate *A. vinlandii* polynucleotide phosphorylase. The fact that they are not incorporated into the products suggests that they may be acting as templates. However, it appears highly probable that the activation by the dialysable material may be via the formation of oligonucleotides from the cyclic-nucleotides as described by Heppel, Whitfeld and Markham[10].

Irrespective of the underlying mechanisms associated with these results, the significant conclusion to be reached is that polynucleotides, through their activation and inhibitory effects, can alter the specificity of the polynucleotide phosphorylase enzyme toward different substrates. This is believed to be brought about not by the addition of monomers to preformed 'primer'[7,8] but by the alteration of the *de novo* synthesis of polynucleotides. It is interesting to speculate whether such a mechanism is at work in the cell in the duplication of ribonucleic acid.

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Neuronal Dye-Sorption as a Histochemical Indicator of Nervous Activity

PROTEIN extracts from electrically stimulated nervous tissue have been reported to undergo changes resembling those of reversible denaturation[1]. Denaturation of protein, however, enhances dye-protein interaction[2]. It was postulated, therefore, that excitation of nervous tissue could be demonstrated histochemically[3].

Since protein in the denatured state has more ionized tyrosine hydroxyl and cysteine sulphydryl groups available than in the native state, iodination, by lowering the *pK* of the protein, will further increase its affinity for basic dyes[4]. Based on these considerations, the following technique has been developed.

Preservation of the living central nervous system tissue was achieved by immediate submersion into liquid nitrogen. Sections 3μ in thickness were cut in the cryostat at $-10°$ C. and mounted on slides by warming with the fingertip. The sections were then treated in 0·5 per cent w/v of iodine in 95 per cent ethanol for 1 min. at room temperature. Descending concentrations of alcohol and finally distilled water were used to remove non-reacted iodine. The sections were then stained in a *tris*-buffered aqueous 1 per cent methylene blue hydrochloride (*C.I.* No. 922 ; Nat. Aniline dye) solution of *p*H 6·9[5]. Treatment with two changes of aqueous 15 per cent sodium chloride solution prior to rinsing in distilled water and mounting in lævulose or preferably in Lillie's cellulose caprate retarded leaching of the dye. Each step has been standardized.

This staining procedure has been applied to the brain tissue of male Sprague–Dawley rats 90–100 days old. Excitation was produced by either electroconvulsive shocks or by intraperitoneal administration of 0·45 mgm. of picrotoxin/100 gm. body-weight. Animals anæsthetized by 6 mgm./100 gm. body-weight of intraperitoneal nembutal served as controls. At the peak of excitation or depression the animals were instantaneously decapitated in such a way that the head dropped into liquid nitrogen. One minute later the heads were cut in frontal sections by a band saw and the brain tissue removed and blocked for sectioning. During this procedure the tissue remained frozen.

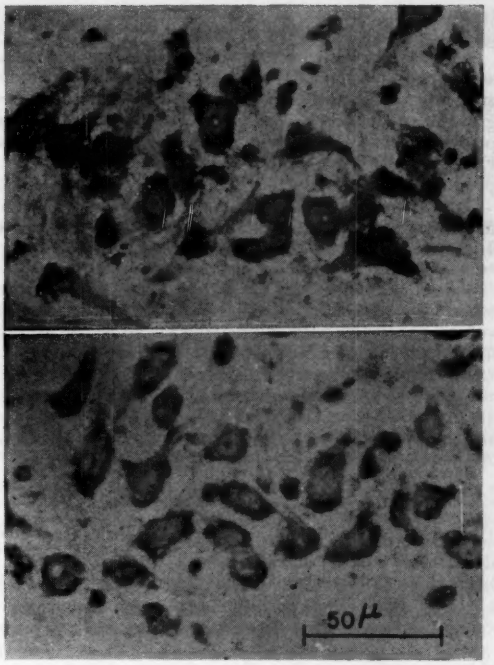

Fig. 1. Rat brain. Large pyramidal cells of hippocampus stained with methylene blue. Above : after excitation with picrotoxin ; below : after depression with nembutal

Excited and control brains were sectioned immediately and mounted on the same slide for comparison. Care was taken to obtain microscopic preparations of corresponding topographical areas.

Surfaces of neurons of excited central nervous system tissue were consistently stained more intensely than surfaces of non-excited, depressed nerve cells. This difference in intensity can only be observed under the microscope (Fig. 1). In addition to the surface of the excited ganglion cell body, the dendrites were also found to be stained to a degree not matched by the control preparations. The nuclei of the nerve cells were not demonstrable. Although glial nuclei are distinctly stained, no consistent differences in staining intensity could be observed between excited and depressed tissue.

Similar findings were revealed in brain sections of twenty-two day old *DBA*–2 mice. In this case excitation was produced by seizures following sonic vibrations of 90 decibels of 1-min. duration. Those litter mates which did not show convulsions under identical conditions served as controls.

Preliminary studies indicate that another phenothiazine derivative, chlorpromazine hydrochloride, follows the same pattern of differential sorption as does methylene blue. When chlorpromazine is used as a 'colourless dye' the differences in sorption must be viewed under a fluorescence microscope.

The nature of the morphological and/or chemical structures involved in the differential sorption of certain phenothiazines has yet to be determined. Pertinent investigations are under way. It is also planned to measure the uptake of dye quantitatively. In connexion with the results reported here it is intriguing to recall that Bodoni[6] observed the

tranquillizing action of methylene blue on agitated mental patients as early as 1899.

This work was carried out with the technical assistance of Thomas P. Biggert. The active interest and helpful suggestions of Dr. B. Pasamanick are gratefully acknowledged. Dr. S. Levine kindly placed the *DBA*–2 mice at our disposal. Smith, French and Kline courteously supplied the chlorpromazine hydrochloride.

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PLANT PHYSIOLOGY

Gibberellic Acid and Growth Correlations

Among other effects, gibberellic acid may alter plant-growth correlations by decreasing the inhibitory action of leaves and storage organs. Previous work[1–3] has shown that species of *Bryophyllum*, *B. crenatum*, *B. verticillatum* and *B. daigremontanum* do not flower if kept continually under either long- or short-day conditions. To induce flowering, long-day plants showing long internodes and petiolated oval leaves must be exposed daily for 10 days to 12-hr. illumination or less, whereas short-day plants with their very short internodes and round, almost sessile, leaves must be exposed for 20 days to a long day of 13 hr. or more and then to short-day conditions for a further 10 days. These plants were therefore described as long-short-day plants[4]. The long-day plants could not be induced to flower by treatment with gibberellic acid. Bünsow and Harder were able to induce elongation and flowering of long-day plants grown under short-day conditions by treatment with gibberellins obtained from the fungus[5] and from unripe bean seeds[6]. The same result was obtained in our laboratory by treatment with gibberellic acid of material of *Bryophyllum crenatum* which had been maintained in the form of rosettes for 12 years by continuous exposure only to short-day conditions. If, however, after treatment with gibberellic acid these short-day plants were exposed to continuous illumination or to long-day conditions, they failed to form flower primordia although they elongated strongly, forming long internodes and new oval petiolated leaves. Thus gibberellic acid can replace the long-day requirement but not the quite specific flower-inducing effect of the short day. Extremely rarely a single flower appeared in the axil of one of the upper leaves of the rosette plants grown under short-day conditions; this was only observed in the spring of 1949 in two out of many hundred plants grown in a 9-hr. day.

Studies have also been made with the long-day plant *Circaea intermedia*, which was the first in which the inhibiting effect of higher concentrations of indole-3-acetic acid on flowering was demonstrated[7]. Weak plants which had remained apparently vegetative even under long-day conditions elongated considerably and flowered when a paste containing 0·5 per cent of gibberellic acid was applied to the stem apex. The same effect is produced by defoliation of the upper part of the stem and so removing the leaves controlling growth inhibitions. Tubers of this plant gathered in autumn and then treated at the apex with gibberellic acid paste formed many long orthotropic filamentous shoots whereas control tubers smeared with pure lanolin either remained dormant or developed slowly growing horizontal stolons with tubers at the end. A similar change in geotropic response also occurred with axillary shoots. When axillary buds in one-node cuttings taken from the basal part of the stem were treated with gibberellic acid paste they developed into long and very thin vertical shoots, whereas the control buds smeared with pure lanolin grew into plagiotropic stolons which tuberized. Thus the correlative growth inhibitions of leaves and tubers, so important for the correlations of growth processes, may be decreased by application of gibberellic acid, which thus replaces the growth-promoting effect of the roots.

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Chlorophyll *a* in some Corals and Marine Plants

Associations of algae and marine invertebrates are common in coral reefs of tropical seas. According to Yonge[1,2] the algal components of corals obtain carbon dioxide and nutrients from the associated animals; but just what advantages of pseudo-symbiosis accrue for the individual animal body has not been clearly demonstrated. Odum and Odum[3] have expressed the view that the algal-coelenterate complex of tropical coral reefs is "a highly integrated ecological unit (comparable to the algal-fungal complex of a lichen) which permits cyclic use and re-use of food and nutrients necessary for vigorous coral growth in tropical 'desert' waters having a very low plankton content". New approaches to this problem of symbiosis have been developed recently by the growing of single-celled zooxanthellæ isolated in pure cultures from marine jelly fish and sea anemones, and demonstration that these are in reality motile dinoflagellates[4]. It has not been clearly shown whether these photosynthetic flagellates contribute food supplies to the animal polyps.

If the trophic structure of a coral reef is a pyramid of biomass with a considerable excess of producing

organisms at the base, then it becomes important to know more about the synthetic potential of the different kinds of primary producers in the reef. Quantitative analysis of pigments can give a measure of the assimilatory potential of green organisms which absorb radiant energy used in photosynthesis. Although various methods, such as the oxygen method and carbon-14 technique, may give precise indications of actual productivity, it is believed that determinations of the chlorophyll *a* content of plankton, algae, corals, etc., may have considerable significance in various problems concerned with productivity and food relations among groups of organisms in the sea.

The present study attempts to assess the chlorophyll potential of a number of different kinds of corals and sea anemones, and to compare them with other photosynthetic systems of algae and flowering plants in a tropical marine environment. Samples of plants and animals were collected near the Marine Laboratory at La Parguera, Puerto Rico, during March 1958. The pigmented portions of these organisms were ground and extracted with 90 per cent acetone in a mortar. After filtration in subdued light, the extracts were made to standard volume, and their absorption was determined at appropriate wave-lengths in a Beckman spectrophotometer. Calculations of chlorophyll *a* per gram of fresh samples were made according to the method of Richards with Thompson[5].

The general results of this survey can be illustrated by results obtained for representative types of organisms, a few of which are shown in Table 1. A summary of the results, arranged according to ten different groups of chlorophyll concentration, is presented in Table 2. Although the distribution of chlorophyll *a* in plants and animals shows overlap in the lowest four groups of Table 2, it is obvious that the concentration of pigment is relatively low in most of the animals studied. Among the various corals and anemones, high chlorophyll values were found in gorgonian corals and in *Condylactis*. Seven species of plants showed chlorophyll *a* contents greater than the maximum observed in any of the animals.

Considerable variation occurred in the different samples of the species which were tested. Thus, five determinations for *Thalassia* gave results ranging from 462 to 655 μgm./gm. of fresh leaf tissue. When the value of 655 for *Thalassia* leaves is recalculated for the whole plant, then the result comes to only 73 μgm./gm. of plant body. If the calculation of pigment on the basis of the whole functional body of plants and animals gives a fair comparison, then many corals would be found to contain chlorophyll *a* in concentrations as high as those of common marine algae and flowering plants. The presence of significant amounts of chlorophyll *a*, along with other pigments in the fauna of coral reefs, is related chiefly to the presence of numerous unicellular algae, which live in association with the polyps. In some of the stony corals, green and blue green algae occur also in the hard structure. Studies on the overall productivity of these algae in the reef community, and symbiotic exchanges between zooxanthellæ and polyps are being continued.

Table 1. Chlorophyll *a* Content (μgm./gm. wet weight) and Pigment Density Ratios in Representative Marine Organisms of Puerto Rico

Organisms	Chlorophyll *a*	D_{480}/D_{665}
Corals		
Briareum asbestinum	97	6·1
Diploria clivosa	25	4·2
Gorgonia flabellum	218	6·0
Plexauropsis crassa	164	5·6
Porites furcata	30	5·4
Anemones, etc.		
Condylactis gigantea	336	4·2
Stoichactis helianthus	115	4·2
Zooanthus sp. (green)	138	5·3
Algae		
Caulerpa verticillata	912	1·5
Cladophoropsis membranacea	217	1·9
Dictyota divaricata	109	2·7
Halimeda tridens	631	1·7
Penicillus capitata	500	1·7
Udotea flabellum	806	1·7
Flowering plants		
Cymodocea manatorum	296	1·6
Diplanthera wrightii	468	1·8
Rhizophora mangle	318	1·8
Thalassia testudinum	655	1·6

Table 2. Distribution of Chlorophyll *a* among 23 Marine Plants and 30 Animals growing in Shallow Waters near La Parguera, Puerto Rico

	μgm. of chlorophyll *a* per gm. of tissue									
	1–100	101–200	201–300	301–400	401–500	501–600	601–700	701–800	801–900	901–1,000
Plants*	5	4	5	2	3	0	2	0	1	1
Animals	23	5	1	1	0	0	0	0	0	0

*Leaves of *Thalassia* occur in the 601–700 class, but chlorophyll *a* of the whole plant body falls into the low 1–100 class, along with the majority of corals.

The ratio of yellow to green pigments may have some significance in estimating the photosynthetic potential of carbon dioxide – light assimilatory systems. According to the data discussed by Richards with Thompson[5], carotenoid absorption may be best observed at wave-length 480 mμ, while chlorophyll *a* shows an absorption maximum near wave-length 665. Hence, we have chosen a simple expression of pigment density ratio = D_{480}/D_{665}, which roughly relates the optical density of the yellow pigments to that of the green pigments. Pigment density ratios for 16 corals, 5 anemones, 18 algae, and 4 phanerogamic plants have been calculated. Typical results are shown in Table 1. The range and average of pigment density ratios among the various groups are as follows: coral range 4·2–6·1, average 4·9; anemone range 4·2–5·3, average 4·6; algae range 1·5–2·7, average 1·8; phanerogam range 1·6–1·8, average 1·7. The leaves of flowering plants and the thalli of green algae commonly show a ratio of about 1·7, whereas the ratio found in corals and anemones is generally greater than 4·6. Studies on the pigment density ratio of the internal green algal bands and the outer brown polyp regions of two corals, *Porites furcata* and *Diploria clivosa*, gave ratios of about 1·6 for the green internal bands and approximately double this value for the polyp region. It appears that photosynthetic capacity is greatest in organisms having a low pigment density ratio, that is, a high proportion of chlorophylls to carotenoids. Carotenoids seem to be unusually abundant in the symbiotic corals and certain kinds of sea anemones.

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BIOLOGY

Reactivation of Heat-inactivated Poxviruses : a General Phenomenon which includes the Fibroma–Myxoma Virus Transformation of Berry and Dedrick

In 1936, Berry and Dedrick[1] reported that active myxoma virus could be recovered from rabbits injected with mixtures of heat-inactivated myxoma and active fibroma virus. Because their experiments were suggested by Griffith's[2] studies on the transformation of pneumococcal types, Berry and later investigators called heated myxoma virus the "transforming agent" and the phenomenon "fibroma-myxoma virus transformation". However, Berry recognized that reactivation of the heat-inactivated myxoma rather than transformation of the active fibroma virus was a possible alternative mechanism.

The term 'transformation', as used in bacterial genetics, has been defined as the heritable modification of the properties of one bacterial strain by an extract derived from cells of another strain. The active material responsible for transformation is deoxyribonucleic acid. There is no evidence that the phenomenon described by Berry and Dedrick is an example of transformation, in this sense. The process is better interpreted as reactivation of the inactivated virus, and we shall use that term henceforth.

Berry's results have been repeatedly confirmed, but further analysis of the mechanism was precluded by the irregular results obtained in intact animals. Recently, Kilham[4] showed that the Berry–Dedrick phenomenon could be demonstrated with regularity with heat-inactivated myxoma virus and active fibroma virus, in cultures of rabbit, monkey and squirrel kidney cells. With collaborators[5] he has described the effects on the heated myxoma virus component of some physical and enzymic treatments.

The definition of several independent genetic markers of vaccinia virus suggested that it might be a more suitable agent for the study of the mechanism of the Berry–Dedrick phenomenon than myxoma and fibroma viruses. The strains of vaccinia virus used for most experiments discussed here were those employed previously for recombination experiments[6]. Their biological characters are set out in Table 1.

Table 1. Biological Characters of Two Strains of Vaccinia Virus (from Ref. 6).

Designation of strain	Morphology of pock on chorioallantoic membrane	Production of hæmagglutinin	Heat resistance	Mouse virulence	Rabbit virulence
RP	hæmorrhagic ulcerated (U^+)	nil (A)	high (T^+)	high (VM^+)	high (VR^+)
V-Led-7N	white non-ulcerated (U)	high (A^+)	low (T)	low (VM)	low (VR)

The appropriate symbols for phenotypes are shown in brackets.

The virulent strain, *RP*, was used as the inactivated agent. After heating for 2½ hr. at 55° C. in 0·1 *M* sodium chloride, 0·01 *M* citrate, at *p*H 7, suspensions which originally contained about 10^8 pock-forming units/ml. were shown to be non-infective when inoculated on the chorioallantoic membrane, with passage of the membrane two days later. All heated material used was inactive by this test, which was the most sensitive available. Mixtures of heated *RP* and active *V-Led-7N* were inoculated intracerebrally in mice, and brains were removed for assay on the chorioallantoic membrane at various intervals. Such brain suspensions yielded clones of virus indistinguishable from *RP* in all the characters listed in Table 1, as well as recombinants between *RP* and *V-Led-7N*. Both the recombinants and *RP* were recovered as early as 18 hr. after inoculation. Since *V-Led-7N* multiplies to a limited extent only in the mouse brain, it was found only in the early stages. This result is directly comparable to Berry's observations, for living virulent virus was recovered from an animal inoculated with a mixture of heat-inactivated virulent virus and a related active attenuated strain. It also showed something that could not be determined with myxoma-fibroma, namely, that some clones of virulent virus resembled the original heat-inactivated virus in all the characters that could be examined, and that others, although highly virulent, were recombinants.

The mouse brain was used to test the capacity of heated *RP* to be reactivated after further physical, chemical and enzymic treatments. Suspensions of *RP* could be heated at 55° C. for more than 7 hr. before the capacity for reactivation was greatly reduced, but this was destroyed in a few minutes at 60° C. In contrast to the report of Smith[7] (for which detailed support has never been published) attempts to dissociate protein and deoxyribonucleic acid have so far been unsuccessful. As Kilham and his collaborators[5] found with myxoma virus, various forms of enzymic digestion of the heat-killed virus (trypsin and deoxyribonuclease at *p*H 11, pepsin at *p*H 3·5, papain at *p*H 7·5 and above) leave the capacity for reactivation intact and associated with particles deposited in the same centrifugal field as vaccinia virus.

Reactivation of heated *RP* was also observed after the inoculation of mixtures of heated *RP* and active *V-Led-7N* into cultures of HeLa cells, and on the chorioallantoic membrane. Use of the chorioallantoic membrane had the advantage that it allowed the direct demonstration of reactivation without the necessity of passage. When about 10^8 particles (originally 10^7 pock-forming units) of heated *RP* were inoculated on the chorioallantoic membrane together with 20–30 pock-forming units of active *V-Led-7N*, about half the pocks seen two days later were of the U^+ type, that is, they resembled those of *RP*. Analysis of 32 clones from U^+ and U pocks showed that nine resembled *V-Led-7N*, 11 resembled *RP* and 12 were recombinants. Using the chorioallantoic membrane, reactivation of heated *RP* has been induced not only by another strain of vaccinia (*V-Led-7N*) as the active agent but also by all the other members of the poxvirus group that have been tested, namely, cowpox, ectromelia, myxoma, fibroma and fowlpox. The reverse process, reactivation of heated *V-Led-7N* by active *RP* and by ectromelia virus, has also been accomplished. The phenomenon is probably a general one, demonstrable with many different combinations of pox viruses, although the ease of recognition of the reactivated component differs. Experiments with several other viruses which produce pocks on the chorioallantoic membrane yielded negative results; no reactivation of heated *RP* was observed when the active virus component was herpes simplex virus, influenza virus strain *WSE*, or Murray Valley encephalitis virus.

The search for recombinants between *RP* and reactivating viruses other than *V-Led-7N* is not yet completed, but reactivation of heated *RP* invariably yielded virus indistinguishable from *RP* in

all the characters set out in Table 1 ($U^+ A T^+ V (M^+ R^-)$). The recovery of such clones from mixtures of viruses as dissimilar as *RP* and fibroma or fowlpox renders unlikely Berry's original idea that the heat-inactivated component 'transforms' the active virus. Reactivation of the heated virus by the active agent is a more plausible explanation. Whether reactivation is accomplished by some sort of genetic interaction, or whether the primary mechanism is repair of some non-genetic damage of the heated virus, with recombination as a possible secondary event, cannot yet be decided.

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Relation of Extracellular to Intracellular Potential Records from Single Cardiac Muscle Fibres

IN the atrial muscle of the heart the rate of depolarization (70–100 V./sec. at 30° C.) is so much more rapid than that of repolarization (less than 1·0 V./sec.) that the two events cannot be confused, and a comparison may be made between records obtained from the inside and outside of a single fibre during the depolarization phase only[1]. Pairs of isolated rabbit atria were mounted horizontally in oxygenated fluid and made to beat at constant frequency in response to electrical stimuli applied to the tip of the left atrium. Contractions were measured with a transducer, and action potentials from the left and right atria were recorded with pairs of small bipolar external electrodes, from which conduction velocity was calculated. Glass microelectrodes, filled with potassium chloride, with tips less than 0·5μ across and resistances of 10–30 MΩ, were introduced under visual control into single fibres on the endocardial surface by a manipulator which could be moved smoothly over distances as small as 2μ.

Fig. 1*A* shows a record of a cardiac action potential obtained with a microelectrode just touching the surface of a fibre. There were four phases: (1) upward positive, (2) fast downward negative, (3) upward positive to just above the base-line, (4) slow return to zero. The electrode was then lowered, and suddenly recorded a resting potential of −74 mV., indicating that it had penetrated a fibre. Fig. 1*B* shows the contraction of the atria (lowest trace), and the intracellular action potential above it. The dotted line represents the zero potential of the electrode when it was extracellular. The vertical trace on the right gives the records from the external bipolar electrodes, and the inset indicates the time to within 0·1 sec.

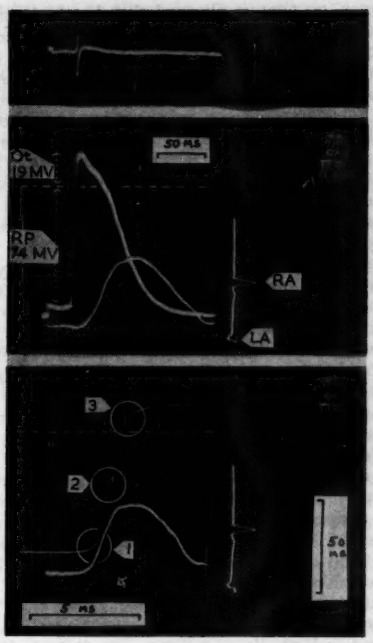

Fig. 1. *A*, Cardiac action potential recorded with microelectrode outside fibre. *B*, Lower trace, contraction; upper trace, intracellular potential; *RP*, resting potential; *Ot*, overshoot; dotted line, zero potential; right-hand trace, external records of action potential from left (*LA*) and right atrium (*RA*); inset, time of exposure at 11 min., 7·1 sec. *C*, As *B*, except that the sweep recording the intracellular potential was faster. (Photographs not retouched)

Comparison of Figs. 1*A* and 1*B* shows that phases (1) to (3) of the extracellular potential were associated with depolarization, and the slow phase (4) with repolarization. In Fig. 1*C* the oscilloscope trace of the intracellular potential had been speeded up

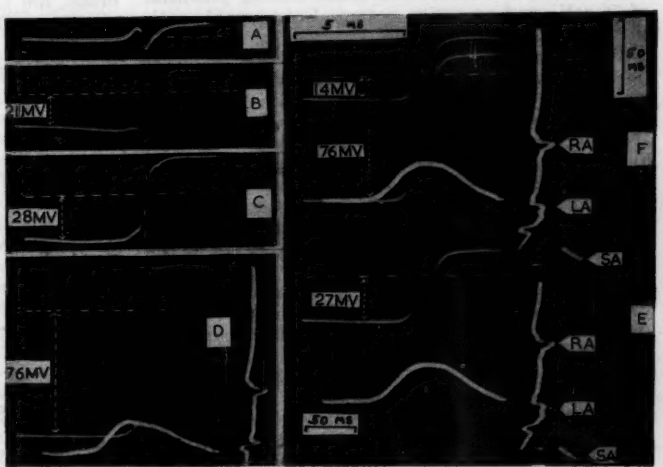

Fig. 2. *A–D*, Four records taken during the introduction of a microelectrode; *E* and *F*, three records taken during consecutive heart beats to show that the negative phase of the action potential recorded when the electrode was extracellular corresponded to phase (2) of the action potential recorded intracellularly; *S.A.*, stimulus artefact. (Photographs not retouched)

23 times, so that it showed the depolarization only, making the first three phases more distinct. The four phases might be interpreted as follows. (1) Membrane active at a distance and active region approaching the electrode. Both extracellular and intracellular records became positive in phase because the indifferent electrode was in contact with the active (outside negative) region via the extracellular fluid. (2) Active region at electrode. The electrode, when extracellular, was on the negative pole of the 'sodium battery'[2], and on the positive pole when intracellular, so that the two records were out of phase. (3) The intracellular record rose to peak positivity as the inflowing depolarizing current came to an end, and the extracellularly recorded potential also became positive as the active region travelled away from the microelectrode. (4) Resting potential restored as membrane very slowly repolarized. It has been remarked that surface records in a volume of fluid bear some resemblance to the second derivative of the monophasic potential[3], but this does not account for phase 4.

As a rule, when the microelectrode was introduced into a fibre, the full resting potential appeared with great suddenness. Occasionally, the resting potential developed slowly or irregularly, and such records were rejected. Some of these records, however, although valueless as measurements of resting potential, were of interest in showing that a record of resting potential was not a reliable criterion for judging whether an action potential was being recorded from inside or outside a cell. Figs. 2*A*–*D* shows four successive records taken during the penetration of a cardiac fibre. The record of Fig. 2*A* was obtained from an electrode just touching the surface of a fibre, and is comparable with Fig. 1*A*, except that the speed of the sweep recording the intracellular potential was faster. In Fig. 2*B*, a resting potential of 21 mV. had developed, yet the action potential could not be said to be of an intracellular type because phase (2) was negative. In Fig. 2*C*, on the other hand, although the resting potential had increased to only 28 mV., the action potential was of an intracellular type since phase (2) was now positive. In Fig. 2*D* the electrode was fully intracellular, and recorded a stable resting potential of 76 mV., and an action potential comparable with that of Fig. 1*C*.

The relation of the phases of the action potential recorded intracellularly and extracellularly is illustrated by Figs. 2*E* and *F*, in which three consecutive heart beats have been photographed, two of them on the same frame. In Fig. 2*E*, during the first beat, although a resting potential of 27 mV. was observed, the action potential was not of an intracellular type since phase (2) of the action potential was negative. Before the next beat the electrode entered the fibre, and the fast positive phase (2) of the upstroke of the action potential corresponds with the negative phase of the superimposed third record, when the electrode had again come out of the fibre. There was no change in conduction velocity between the beats, because the two superimposed records in Fig. 2*F* of externally recorded action potentials from the left and right atria (to the right of the frame) do not differ measurably from the single similar record in the preceding frame (Fig. 2*E*).

Microelectrodes inside single fibres of frog skeletal muscle do not record to a significant extent potentials occurring in surrounding fibres[4,5], and a tracing such as that of Fig. 1*C* provides no evidence that this is not also true of cardiac fibres. The simplest explanation for the records of Fig. 2, therefore, is that the electrode was at first in a fibre damaged to the extent that the resting potential was abnormally low and the action potential absent (Fig. 2*E*) or attenuated (Fig. 2*C*), but that the electrode was also recording as an external electrode the action potential in a subjacent fibre which it eventually entered (Figs. 2*D* and *F*). In any event it is apparent that the observation of a d.c. potential is inadequate evidence that an action potential superimposed on it is being recorded intracellularly, an assumption which can be made only if (*a*) there is a large and stable resting potential, (*b*) there is no negative component in phase (2) of the action potential.

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Development in a Mutelid Lamellibranch

Although the life-history of lamellibranchs of the family Unionidae has been known for almost a century, little has been recorded concerning members of the related Mutelidae. This taxonomically ill-defined family has representatives in the freshwaters of Africa, South America and Australia. A glochidial stage similar to that of the Unionidae is said to occur in the development of some reputed mutelids in Australia. At least one South American species produces a peculiar larva to which the name lasidium has been given but its subsequent development is unknown. Nothing has been recorded regarding the development of the African species.

Recent studies have elucidated the life-history of the African species *Mutela bourguignati* (Ancey) Bourguignat 1885, which is depicted in Fig. 1. The stages illustrated are referred to in the text by the relevant letters. The adult female produces numerous minute larvæ (*a*) which, while still within the egg membrane, are brooded in the inner demibranchs of the ctenidia. These larvæ are in some ways similar to the typical molluscan veliger but differ in certain respects, most of the differences being correlated with

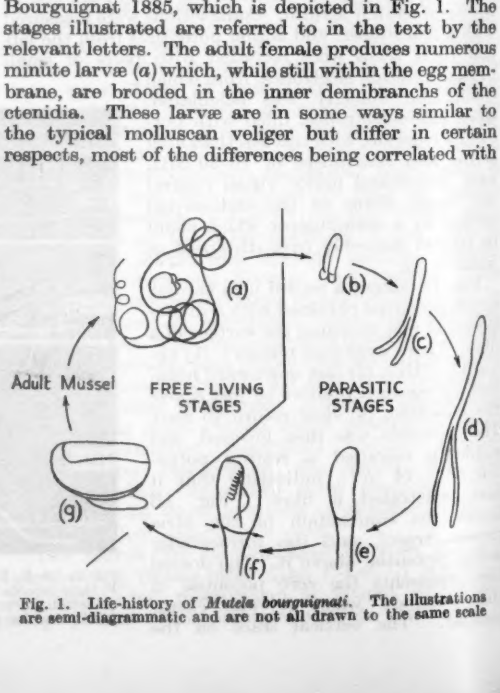
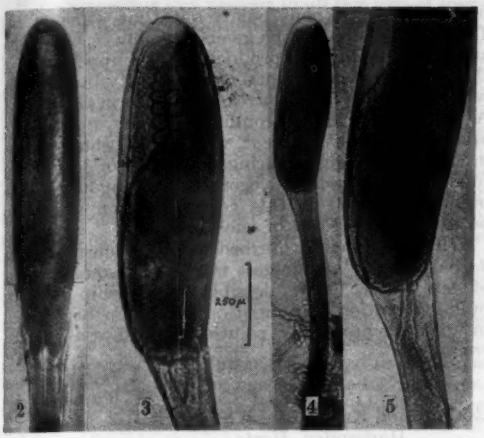


Fig. 1. Life-history of *Mutela bourguignati*. The illustrations are semi-diagrammatic and are not all drawn to the same scale



Figs. 2 and 3. Posterior (distal) extremity of fairly advanced post-larvæ in which, although still completely enclosed within the post-larval cuticle, the rudiments of certain adult organs are well defined. Fig. 4. The same specimen as that shown in Fig. 3 but showing more of the post-larval tube. (The objects adhering to the lower part of the tube are sessile ciliate protozoans which often colonize this region.) Fig. 5. Region where, at a later stage of development, the young mussel and the stalk will part company. In all the photographs, and most clearly in Figs. 2 and 5, the anterior prolongations of the mantle, which extend into the anchoring tubes, can be seen

the subsequent period of parasitism. Most remarkable of these larval features is the possession of an extremely long filamentous tentacle which resembles the tentacle of the glochidium of the Unionidae but is very much longer, being more than 70 times that of the larva itself.

These larvæ are shed through the exhalant siphons of the parent, and such as locate a host attach themselves to the cyprinid fish *Barbus altianalis radcliffi* Boulenger, usually, but not exclusively, to the fins. Attachment is probably effected initially by means of minute hooks with which the larva is provided posteriorly. After establishment on the host a remarkable transformation takes place. The tentacle is lost, and the larval shell, which at this stage consists merely of a thin pellicle and shows no signs of calcification, folds into a minute bivalved structure in which fusion occurs in the mid-ventral line to give, ultimately, a completely enclosed structure. This process is reminiscent of what occurs during metamorphosis of the Scaphopoda, but is carried even further in that both ends of the folded shell are sealed. Two outgrowths arise from the anterior end and penetrate the superficial tissues of the host (*b*). Growth of these structures results in the formation of two completely enclosed tubes (*c*) which serve as organs of attachment and through which nutritive substances are presumably absorbed from the host. With the exception of these structures the post-larva develops outside the host tissues. As a result of the fusion process described, the body of the post-larva is enclosed in a sealed tube the nature of the non-calcareous walls of which is being investigated. This tube grows to a length of about 3 mm. Within its distal extremity the adult mussel is eventually formed. Running from the distal mass of tissue through the proximal part of this tubular structure, and separated from each other by a longitudinal septum of the same material as the outer walls, are two strands of tissue which, as revealed in later stages of development (Figs. 2–5), are morphologically anterior extensions of the mantle. These extend into, and to the extremities of, the anchoring tubes. At this stage the parasitic post-larva is not only unlike a mollusc but also, from its external appearance, could easily be mistaken for a fungal hypha.

Enlargement of the distal extremity and differentiation within it (Fig. 1*d* and *e* and Figs. 2–5) results in the formation of adult organs—mantle, foot, ctenidia, adductor muscles, etc.—and the organic matrix of the adult valves begins to be formed. The periostracum is secreted from the inner face of the outer mantle fold in the manner typical of lamellibranchs. At first this primitive shell material forms a layer inside, and closely adpressed to, the post-larval tube; but subsequent growth and secretion result in a longitudinal split of the tube in that region of the post-larva where the adult structures are forming and along what will be the mid-ventral line of the adult mussel. The post-larval tube material, however, continues to embrace the rudimentary adult valves and to strengthen the ligament region (*f*). Calcareous material is deposited in the valve rudiments and the gut opens to the exterior so that, even before it leaves its host, the mussel can, and does, feed by ciliary action in the typical lamellibranch manner. At this stage, which represents the termination of the parasitic phase of the life-cycle, the organism is clearly recognizable as a typical bivalve attached to the end of a long stalk.

Rupture of the tubular outgrowth at a point near the anterior end of the newly formed valves (indicated by arrows in Fig. 1*f*) results in the release of the young mussel (*g*), which falls to the bottom and commences an independent existence. At the time of its release it has a length of rather less than 1·5 mm. The stalk remains attached to the host.

Not only is such an ontogenetic sequence highly specialized, but also both larval and post-larval stages, and particularly the latter, exhibit features which are unique within the Mollusca. Anatomical studies of the various stages of development are now in progress and a detailed illustrated description of these and a more complete account of the life-cycle will be published in due course.

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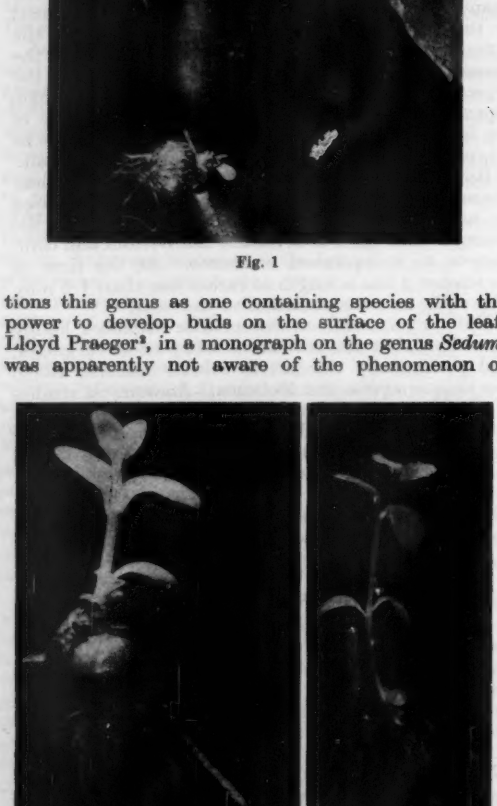
Bulbil Formation in *Sedum telephium*

A vegetative plant of a species of *Sedum*, afterwards identified as *S. telephium*, was collected in a hedgerow on Rothamsted Farm on June 9, 1958, and placed with its roots in a jar containing tap water to keep it alive for further study. The jar stood on the laboratory window-sill for three months, when it was observed that, although the specimen failed to flower, bulbils had formed in the axils of the upper leaves—positions in which flowering stems would normally arise. Some axils contained several bulbils as shown in Fig. 1. Each bulbil bore scale-leaves and numerous white roots, each of which had a conspicuous dark root cap. When separated from the parent plant and placed on damp filter paper, each bulbil quickly developed into a new plant. Fig. 2*a* shows such a plant after six days growth and Fig. 2*b* after 13 days growth respectively. At the first two or three nodes the leaves were simple and opposite, but the fourth node produced irregularly dentate leaves typical of the adult plant.

Bulbil formation seems not to have been previously reported in the genus *Sedum*, although Godron[1] men-

Fig. 1

tions this genus as one containing species with the power to develop buds on the surface of the leaf. Lloyd Praeger[2], in a monograph on the genus *Sedum*, was apparently not aware of the phenomenon of bulbil formation, but points out that some of the *Telephium* section of the genus, notably *S. viviparum* and *S. verticillatum*, produce in the upper part of their annual stems numerous small leafy buds which, when the stems fall, root readily and form new plants. A somewhat similar condition occurs in *Crassula multicarva* and *C. cordata*[3], where small shoots in the inflorescence region bearing two pairs of leaves can function as organs of vegetative propagation. The bulbils described in this communication appear to be an extreme form of such shoots.



(a) (b)

Fig. 2

Why this specimen formed bulbils is uncertain, but its only supply of minerals was from a small amount of tap water. The possible relationship between bulbil formation and nutrition will be further studied in experiments using solutions of known mineral content.

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Ecology of *Mucor ramannianus* Moller

Mucor ramannianus is one of the commonest fungi occurring in certain acid, sandy soils, and often, in some horizons, may be the only fungus isolated by standard plating methods. In grassland soils at Lakenheath Warren it was confined to those of low *p*H value[1], and Thornton[2] found it to be very common in podsolized soils beneath *Calluna vulgaris* L., but absent from soils of similar derivation beneath mixed oakwood. *M. ramannianus* was a characteristic fungus in six out of nine acid, heath soils examined by Jefferys, Brian, Hemming and Lowe[3], who suggested that its presence was related to the existence of a well-marked iron-pan in the *B* horizon. McLennan and Ducker[4,5] have further suggested that a predominance of *M. ramannianus* is indicative of acid, sandy soil types with a well-defined pan layer. Detailed studies, primarily of a physiological nature, have been described by Müller[6], who concluded that not only the presence of growth factors but also the hydrogen ion concentration of the soil and source of carbon would play an important part in limiting its distribution. *M. ramannianus* was among the commonest species encountered by us in soils and on roots during detailed surveys of the mycofloras of some British dune systems[7] and *Calluna*-heathlands[8]; its distribution was closely related to certain soil conditions which are discussed below. These observations are presented in view of the fragmentary and sometimes conflicting data available on the ecology of this interesting species.

The succession of soil fungi from open sands of the shore to fixed dune in Britain typically culminates in a community resembling that of acid heathland or alkaline grassland. *M. ramannianus* was isolated by the soil plate method[9] from one or more of the acid semi-fixed or fixed zones in all such dune systems investigated, but from none of the alkaline or young acid dunes. The acid dune zones in which *M. ramannianus* attained a frequency of at least 50 per cent were dry dune-heaths dominated by *Ammophila–Carex–Calluna*, *Calluna*, or *Calluna–Pteridium* communities.

M. ramannianus was present in all but two of fourteen distinct inland *Calluna*-heath soils from six counties of England and Wales, but was of very infrequent occurrence in several of these. There was a marked relationship between its frequency of occurrence and the moisture relations of the habitat as indicated by the surface vegetation; indeed, *M. ramannianus* appeared to be less tolerant of wet conditions than *Calluna* itself, and was of infrequent occurrence or absent from soils where plant communities indicating damp soil conditions occurred, that is, *Calluna* in association with *Molinia*, *Erica tetralix* and *Salix repens*.

In both acid dune and inland heath soils the frequency of occurrence increased with depth to a maximum in the humus-poor or leached, subsurface sands. Within the acid dune succession the first distinct subsurface population was found in the dune heath profiles when *M. ramannianus* and *Penicillium restrictum* were characteristically associated together at a soil depths of 6–12 in., often in the absence of other species. Similarly, in dry, inland heaths, *M. ramannianus* was characteristically predominant in specifically poor mycofloras of the A_2 horizons. The lower limit of vertical distribution in dune heaths was often clear-cut although the sand above and below it appeared similar. This phenomenon was occasionally observed in inland heaths where the water-table was near the surface; but in heath soils lacking vertical zonation, *M. ramannianus* was generally present at all depths sampled, and in podsolized soils its vertical distribution was sharply checked at the illuviated horizons. The low frequency of isolation of *M. ramannianus* from surface soils may have resulted from the vigorous competition of other species on plates prepared from these soils. Early excision, from soil plates, of fungi with a rapid and spreading growth habit did not, however, result in an increase in the number of *M. ramannianus* colonies.

Root isolation studies, employing a root washing device similar to that described by Simmonds[10], indicated that *M. ramannianus* was a common and active root-surface inhabitant. It occurred with frequencies of 25 per cent and 50 per cent on *Ammophila* roots from the A_1 and subsurface soils respectively in acid-fixed dunes, but was not recorded on such roots from the fore-dunes. The fungus was also very common on *Calluna* roots from the A_2 and B_1 horizons of an inland dry heath soil. Thrower[11], however, has concluded, from the studies of the rhizospheres of Australian heath plants, that *M. ramannianus* was predominantly an inhabitant of the free soil and infrequent in the root region.

Our investigations indicate that *M. ramannianus* is a prominent member of acid, semi-fixed and fixed dune heath and inland dry heath communities. Since it was isolated readily by a modified slidetrap method[12] from both types of habitat, it is probable that the fungus commonly exists as an active mycelium in these soils. The calcifuge habit of *M. ramannianus* was indicated by its intolerance of alkalinity in culture media, where growth and sporulation were at a maximum at *p*H 3–4 and sparse at *p*H 7 (Brown[7]) and by its absence from calcareous dune systems. A similar influence of hydrogen ion concentration on its growth has also been demonstrated by Müller[6]. The distribution of *M. ramannianus* in a wide range of heathland soils and its absence from wet dune lows and slacks indicated an intolerance also of damp soil conditions, or some other associated factor. (It should be noted that neither of these factors appears to be completely limiting since Stenton[13] isolated *M. ramannianus* from slightly alkaline, peaty soil at Wicken Fen.) Throughout the investigations there was no indication that the occurrence of *M. ramannianus* was related to the presence of illuviated horizons in the soil profiles, as has been suggested by Jefferys *et al.*[3] and McLennan and Ducker[4,5]; indeed, the results presented by Jefferys *et al.* may be interpreted in relation to soil moisture status (as judged by the nature of the surface flora) as readily as to the presence of a well-marked iron-pan in the *B* horizon.

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Perfect Stage of *Keratinomyces ajelloi*

Keratinomyces ajelloi was first described in 1952 by Vanbreuseghem[1], who isolated it from soil by means of a technique[2] which consists of placing hair on the surface of the soil in a Petri dish as 'bait' for keratinophilic fungi. Since this original isolation, the fungus has been reported many times and is clearly a common inhabitant of soil and of widespread distribution.

Recently, fourteen samples of soil from eight districts in Glasgow and the west of Scotland were investigated for keratinophilic fungi. *K. ajelloi*, in most cases with certain other keratinophilic fungi, was isolated from thirteen samples. The remaining sample did not contain any keratinophilic fungi.

The soil samples were collected into polythene bags from the top 4 in. of cultivated land or immediately beneath the turf of fallow land and processed within a few days. Essentially the technique employed was that of Vanbreuseghem; but the soil was baited with horse mane or tail hair, child's hair, shreds of cow horn and feathers, as well as with adult human hair. The Petri dishes were incubated at 28° C. and examined at intervals over a period of 5–6 weeks.

K. ajelloi could be identified by microscopic examination, and inoculations made for culture as early as one week after baiting. By the end of the

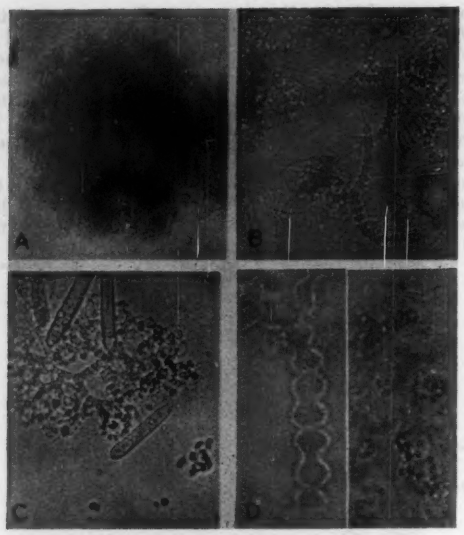

Fig. 1. *A*, mature cleistothecium, mount lactophenol (× 65); *B*, peridium showing characteristic branching and spiral appendages, mount cotton blue in lactophenol (× 265); *C*, macroconidium formed from peridial hyphæ, mount lactophenol (× 250); *D*, cells of peridial hyphæ (× 625); *E*, asci (× 625)

third week, creamy yellow globose structures had developed on certain of the baits on the surface of seven of the soil samples from three districts. Microscopic examination revealed these to be cleistothecia. They were closely associated with *K. ajelloi*, and it was shown by culture of complete cleistothecia and of single ascospores that they were in fact the perfect stage of this fungus. There was no evidence of sexual reproduction in the six isolates of *K. ajelloi* from the soil of the remaining four districts.

The cleistothecia (Fig. 1*A*), which measure 300μ–900μ in diameter, have a peridium of pale yellow hyphæ with a characteristic pattern of claw-like branches (Fig. 1*B*) entangled to form a close network. The individual cells (Fig. 1*D*) are dumb-bell shaped with thick echinulate walls and many of them bear long, smooth-walled spiral appendages (Fig. 1*B*) or occasionally macroconidia of the characteristic *Keratinomyces* form (Fig. 1*C*). The asci (Fig. 1*E*) which are formed in large numbers are sub-globose, and each contains eight bright yellow ascospores with finely echinulate walls.

Sexual reproduction is apparently influenced by a number of factors. It was never observed in culture on agar media, and on primary isolation cleistothecia were formed regularly on only certain of the keratinaceous baits. On horse hair and child's hair they were always produced abundantly, whereas on adult human hair on the same soils they formed sparsely on only one occasion after an incubation period of 6–8 weeks.

A number of attempts were made to ascertain the conditions necessary for sexual reproduction. Complete cleistothecia were inoculated on autoclaved horse hair which was maintained in a humid atmosphere; but a few cleistothecia were produced on only two of the many occasions on which this was done. Unsatisfactory results were also obtained from a large series of experiments in which sterilized soil previously inoculated with cleistothecia or with agar cultures was baited with horse hair. Eventually regular sexual reproduction on horse-hair bait was achieved by inoculation of unsterilized soil with cultures of cleistothecial strains. For this purpose the soil known to be negative for keratinophilic fungi was used. Attempts were then made to induce sexual reproduction in those strains which had not formed cleistothecia when isolated originally. They were inoculated singly and in combination with one another or with single ascospores strains into the unsterilized negative soil baited with horse hair. However, cleistothecia were never formed. Three single ascospore cultures (*A*, *B* and *C*) were also treated in this way. Cleistothecia formed regularly in all the Petri dishes inoculated with strain combinations *AB* and *AC* but not with combination *BC* or with single strains.

It would therefore appear that *K. ajelloi* is heterothallic and that compatible strains will reproduce sexually under certain conditions which are fulfilled by certain keratinaceous baits on the surface of unsterilized soil. However, certain strains may have lost their ability to reproduce by sexual means. In this small series it has been noted that those strains which were 'non-cleistothecial' on original isolation and in which cleistothecial formation could not be induced failed to produce the typical vinaceous red pigment when grown on glucose peptone agar. All cleistothecial strains formed pigment, as did the monospore cultures. There is, as yet, insufficient evidence to assume that pigment production is indicative of ability to reproduce sexually, but this and other aspects are being further investigated.

We are indebted to Mrs. E. Forbes for assistance with minospore isolations and to Miss E. Brown for technical assistance.

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Inactivation of Organo-Mercurial Fungicides in Groundwood Pulp made from Logs stored in Salt Water, and the Possible Role of Sulphur Compounds

Recent experiments conducted in our laboratories have shown that there is a correlation between the efficiency of organo-mercurial fungicides, used for the preservation of moist groundwood, and the concentration of certain sulphur compounds in the logs from which the groundwood is produced. These sulphur compounds are considered to arise from the reduction of inorganic sulphates, absorbed from salt or brackish water during storage, by halophilic strains of *Desulphovibrio desulphuricans*. These bacteria have been isolated from the sapwood of logs stored under natural conditions in sea water for a year. Chemical and microbiological studies have been made on samples of pulp, and on logs obtained from a number of Scandinavian groundwood mills after storing for different periods in fresh water, brackish water and sea water. In every case high sulphur contents, expressed as sulphidic and reducible[1] sulphur, were found both in pulp samples containing biologically inactive fungicide and in the sapwood of the logs from which they had been made. Conversely, pulps having a high residual content of

biologically active fungicide (and consequently a good record of freedom from fungal infection during storage) contained considerably less sulphidic and reducible sulphur, as did the corresponding logs. The latter logs and groundwood pulps came from mills where the wood is stored for 1–2 years in fresh water prior to grinding; the former from mills on or near the coast where the logs are kept in sea water or brackish water for similar periods of time. It has, in fact, been found that after only three months soaking of logs in sea water, mercurial fungicides are de-activated at once on addition to the resultant pulp. It has been shown in the laboratory that the de-activating property progresses farther into the heart of the log with increasing length of immersion.

(In this work sulphidic sulphur has been defined as hydrogen sulphide liberated by hydrochloric acid, and reducible sulphur as sulphur present in the sample and reducible, under the conditions of test, to hydrogen sulphide.)

Sea water itself has been shown to have no adverse effect on either phenyl mercuric acetate or phenyl mercuric acetate-8-hydroxyquinoline (P.M.A.-oxine) mixtures. Thus, from these observations and the presence of *D. desulphuricans*, the inference may be drawn that the absorption by logs of inorganic sulphates is followed by reduction to certain sulphur compounds, which are able to bring about the formation of biologically inactive mercury compounds when the fungicide is added to the pulp. The interaction between mercury and its salts and hydrogen sulphide produced in soil by the bacterial reduction of sulphates has been reported by Booer[2]. Whatever the precise mechanism of this final step may be, the de-activating effect has been found to be immediate with both commercial and laboratory-made pulps derived from sea water-soaked logs.

The exact forms in which these sulphidic and reducible sulphur compounds are present in the wood (or pulp) is still being investigated; it is possible that hydrogen sulphide liberated by bacteria may react to form thiolignin[3] and this point is under investigation.

A detailed account of this work will eventually be published elsewhere.

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GENETICS

Stable Equilibrium at a Sex-linked Locus

Bennett[1] has shown that a stable selectively balanced polymorphism between two alleles can exist at a locus in the *X*-chromosome under conditions which are different from those determining stable equilibrium in the autosomal situation. In particular, it is neither necessary nor sufficient, in the sex-linked case, for the heterozygote in the homogametic sex to be at a selective advantage with respect to both homozygotes. It has previously been found[2] that this principle of heterozygosity operates in the case of multiple allelic systems in autosomes in the following form: a necessary condition for stable equilibrium is that the viability of each homozygote be less than the mean viability of the zygotic population at equilibrium. It seems at first sight that no such heterozygosity principle operates in the sex-linked situation. It will, however, be shown here that it does do so in a somewhat modified form.

It is convenient here to use homogeneous parameters to express relative viabilities, rather than Bennett's method of expressing them relative to the viability of the heterozygote. Thus we may have, for two alleles A,B (taking females as the homogametic sex):

	Females			Males	
Genotype	AA	AB	BB	A	B
Relative viability	a	h	b	a^*	b^*

In any one generation let the frequencies of A,B be p,q respectively in females, and p^*,q^* respectively in males, so that:

$$p + q = 1, \quad p^* + q^* = 1 \tag{1}$$

Then in the next generation (after random mating) we obtain genotypic frequencies as follows:

	Females			Males	
Genotype	AA	AB	BB	A	B
Frequency before selection (zygotes)	pp^*	$pq^* + p^*q$	qq^*	p	q
Frequency after selection (mature adults)	$\frac{app^*}{T}$	$\frac{h(pq^* + p^*q)}{T}$	$\frac{bqq^*}{T}$	$\frac{a^*p}{T^*}$	$\frac{b^*q}{T^*}$

$$\left.\begin{aligned} \text{where } T &= app^* + h(pq^* + p^*q) + bqq^* \\ T^* &= a^*p + b^*q \end{aligned}\right\} \tag{2}$$

For equilibrium of the gene frequencies in the two sexes the gene ratios must have the values:

$$\text{in females: } R = \frac{p}{q} = \frac{h(a^* + b^*) - 2bb^*}{h(a^* + b^*) - 2aa^*} \tag{3}$$

$$\text{in males: } R^* = \frac{p^*}{q^*} = \frac{a^*}{b^*} R \tag{4}$$

Moreover, the equilibrium is stable if and only if the conditions:

$$\frac{h(a^* + b^*)}{2} > aa^*, \quad \frac{h(a^* + b^*)}{2} > bb^* \tag{5}$$

are satisfied.

It is clear from (3) and (5) that for the homogametic sex the conditions determining equilibrium and its stability are identical with those governing two alleles A,B at an autosomal locus in which the relative viabilities of the genotypes AA, AB, BB are

$a' = aa^*$, $h' = \frac{h(a^* + b^*)}{2}$, $b' = bb^*$ respectively.

If we now consider females only with the modified relative viabilities a', h', b', the principle of heterozygosity emerges in the following way. The mean viability V_e of female zygotes at equilibrium can be written:

$$V_e = a'pp^* + h'(pq^* + p^*q) + b'qq^* \tag{6}$$

where p, q, p^*, q^* satisfy (1), (2) and (3).

Now,

$$V_e = a' + \left\{(b' - a') + (h' - a')R\left(1 + \frac{a^*}{b^*}\right)\right\}qq^*$$

$$= a' + \left\{(h' - a') + \frac{a^*}{b^*}(h' - b')\right\}qq^*$$

using (2) and (3).

If the equilibrium is stable then (5) holds also and we have:

$$V_e > a' = aa^* \quad (7)$$

and similarly: $V_e > b' = bb^*$ (8)

These conditions are necessary for the existence of a stable equilibrium, and constitute the heterozygosity principle in its modified form.

It is worth while noting a second form of the new heterozygosity principle which in the case of an autosomal allelic system has proved a valuable interpretative guide. In the present case the second form of the principle is entirely equivalent to either of the formulations given above, namely, (i) $h' > a'$, b'; (ii) $V_e > a'$, b'.

If in the mature females the genotypic frequencies of AA, AB and BB are u, v and w respectively, then under random mating without selection the index:

$$\varphi = \frac{v^2 - 4uw}{(2u + v)(v + 2w)} \quad (9)$$

(which is in fact minus the inbreeding coefficient F) is zero.

In the polymorphism considered here, φ is equal to a positive multiple of:

$$h^2(R + R^*)^2 - 4abRR^* = R^2\left\{h^2\left(1 + \frac{a^*}{b^*}\right)^2 - 4ab\frac{a^*}{b^*}\right\}$$

$$= \frac{4R^2}{b^{*2}}\left\{h'^2 - a'b'\right\}$$

and this is positive by the conditions (5) for stability. This indicates that a condition for stability is that the female heterozygotes are present in excess of Hardy–Weinberg expectations. Thus the principle of heterozygosity holds in this case in a very definite and graphic form.

The above treatment of a sex-linked locus in terms of the autosomal case can be extended to multi-allelic loci in the X-chromosome. This will be discussed elsewhere.

I wish to thank Dr. A. R. G. Owen for suggesting this problem and for much helpful advice and encouragement; and the Royal Commission for the Exhibition of 1851 for the award of an Overseas Science Research Scholarship.

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Feb. 6.

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Congenital Transmission of Toxoplasmosis through Successive Generations of Mice

Congenital transmission of infectious agents—viruses, rickettsiæ, spirochætes and piroplasms—is not uncommon in arthropods and may pass through many generations. It is much less common in mammals in which, when it occurs, it usually stops at the second generation. In mice, however, the virus of lymphocytic chorio-meningitis can be transmitted indefinitely[1], and there are claims that in man syphilis can pass to the third generation[2].

The protozoon *Toxoplasma* was reported by Wildfuhr[3] to have been congenitally transmitted to the third generation of rats. Recently, I have observed its transmission to successive generations of mice—five so far.

The experiments started with infection by subcutaneous inoculation of Swiss mice, six between the eighth and sixteenth days of pregnancy, one 2 days after a mating, which did not result in obvious pregnancy, and one 5 days after delivery. The inoculation consisted of between 30 and 600 cysts of a strain of *Toxoplasma* of low virulence (rabbit *A*), originally isolated from a naturally infected, but apparently healthy, laboratory rabbit. Although congenital infection with the strain shortens the lives of mice, reduces the number of litters they produce and the number reared to maturity in each litter, artificial inoculation does not make them obviously sick unless done during pregnancy or lactation. To this may be ascribed the heavy mortality in most of the first litters of the original mice, as a result of which in only one of these did a female survive to continue the line. In the other instances the lines derived from second litters.

All eight mice transmitted *Toxoplasma* infection to some of the second generation. Two lines of infection stopped there; one because the only infected female of the second generation proved infertile and the other because the one surviving female was killed when a very low antibody titre was, erroneously, taken to signify absence of infection. Four continued to the third generation at which one stopped because only males were infected and two because the females did not become pregnant. The fourth still goes on. Also continuing, and infected, are one line in the fourth and one in the fifth generations.

In all there have been 33 first or subsequent pregnancies in the original mice or their descendants. Except in three of these pregnancies, some of (on average half) the litter have been infected.

The presence of infection in a litter was determined by killing the males when 6 weeks old, making emulsions of their brains and examining them for *Toxoplasma* cysts. If these were found, the females were reared for a further 4 weeks and then mated to non-infected males. Before this their blood serum was in some instances examined for *Toxoplasma* antibodies, and when they were eventually killed all their brains were examined for cysts.

It would seem likely that infection is transmitted, for the most part, before birth, for toxoplasms were isolated by inoculation of surgically removed fœtuses into uninfected mice.

Of course it can also be transmitted by the milk, as shown by Eichenwald[4]. This I can confirm as the result of experiments in which mice were inoculated during lactation. Infection does not appear to be transmitted by droplet, fæces, urine or by copulation, for 10 female mice and their 33 litters, comprising 216 offspring, did not acquire infection from 10 infected males with which they were caged for 4–8 months.

There are many puzzling features in *Toxoplasma* infection of laboratory mice. Natural infection is fortunately almost unknown, yet mice are very susceptible to inoculation, to intranasal instillation of proliferative forms, or to feeding with cysts. Cannibalism is the only way yet known by which adult mice can be infected naturally, yet when infection is

established it passes fairly readily from generation to generation. Nor is it necessary, as Eichenwald[4] believed, for infection to be recent and acute. Twenty-three of the infected litters were borne of mothers themselves congenitally infected, and in one instance a congenitally infected mother produced congenitally infected offspring even in her fourth litter.

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Feb. 9.

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An Induced Mutant in *Triticum carthlicum* with the Diagnostic Feature of *T. vavilovi*

The spike of *Triticum vavilovi* Jakub. is distinctive in that the internodes of the rachilla are more elongate and the number of rachilla nodes increased compared with other hexaploid wheats. The elongated rachilla internodes sometimes so reduce the extent to which the florets overlap as to give the spikelet two or three distinct levels (Fig. 1).

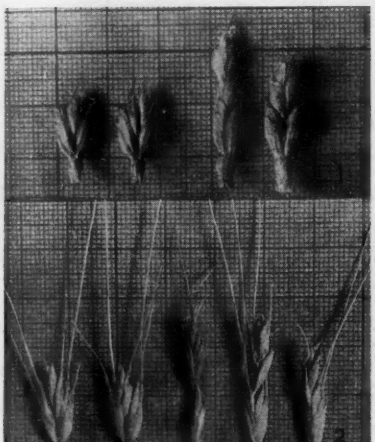

Fig. 1. Two *T. aestivum* normal (left) and two *T. vavilovi* spikelets with several levels (right)

Fig. 2. Two *T. carthlicum* normal (left) and three *T. carthlicum* mutant spikelets (right) with several levels

In 1955, as part of a larger programme, dry seed of genetically pure *T. carthlicum* Nevski (= *T. persicum* Vav.), a tetraploid wheat, was treated with a dose of 10,000 r. X-rays. In the X_3 generation, examined in 1957, a mutant was found which strikingly resembled *T. vavilovi* (Fig. 2). This type had not been present in the X_1 or X_2 generations, but 21 plants in the X_4 progeny of the mutant plant all repeated the mutant structure.

The most important divergences of this mutant from the original *T. carthlicum* stock are compared in Table 1 with the differences between *T. vavilovi* and *T. aestivum* var. Newthatch, the hexaploid common wheat. The total extent of the difference between the *T. carthlicum* mutant and the parental stock is readily ascertainable but the extent of the original difference between *T. vavilovi* and *T. aestivum* cannot be exactly determined because of the present variation in the latter species. The two species are constantly differentiated only by the characters listed in Table 1. Many of the spikelet types seen in the investigation of the genetical constitution of *T. vavilovi* by Singh, Anderson and Pal[1] were observed in this relatively small range of mutant material.

Table 1

Character	Hexaploids		Tetraploids	
	T. aestivum	*T. vavilovi*	*T. carthlicum*	Mutant
Spike	normal	pseudo branch	normal	pseudo branch
Spikelet number of levels	1	2, sometimes 3	1	2, sometimes 3
Rachis at the spikelet bases			glabrous	pubescent
Rachilla number of nodes	5	7	5	7
Internodes	short	elongated	short	elongated
Glume, outer glume			bearded	beardless, with beak-shaped end
Inner glume, arista			long	shorter

According to Isenbeck and Rosentiel[2], Vavilov does not touch upon the genetical bases of *T. vavilovi*. Isenbeck and Rosentiel suggest that the development of this form may be explained as a one-step mutation from *T. aestivum*. This explanation is not excluded by the data of Singh, Anderson and Pal[1] from crosses between *T. vavilovi* and several *Triticum* species. Further, it appears that there is no genome difference between *T. aestivum* and *T. vavilovi*. This hypothesis is confirmed, at the tetraploid level, by a mutant analogous with *T. vavilovi* which actually originated in a single mutational step. In this case of parallel mutations, it may be supposed that since the final outcome was identical the pathways were also identical.

The genetic foundations of the mutant in *T. carthlicum* will only be established by crossing experiments already commenced. The mutation of a single gene to the recessive condition would be the most simple explanation for segregation occurring in X_3 and for the mutant to be true breeding in X_4.

Summing up, the 'vavilovoid' mutant may be of interest in that it illustrates parallel variation and parallel genotypes at the tetraploid and hexaploid level. Moreover, it throws light on the origin of *T. vavilovi* and confirms the supposition of Isenbeck and Rosentiel. Finally, it raises a taxonomic problem, namely, that if *T. vavilovi* is treated as a distinct species as classified by Vavilov[3], by Jakubsiner[3] and by Singh, Anderson and Pal[1], then it must be accepted, at the tetraploid level, that a new species arose from *T. carthlicum* in a single mutational step. Alternatively, if *T. vavilovi* is treated as a subspecies of *T. aestivum*, as it is classed by Sears[4] and by Schiemann[4], subspecific variation must have originated in a single step.

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Nov. 24.

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[2] Isenbeck, K., and Rosentiel, K., "Handbuch der Pflanzenzüchtung", **2**, 289 (Parey, Berlin and Hamburg, 1950).
[3] Vavilov, N. I., "The Origin, Variation, Immunity and Breeding of Cultivated Plants", **13**, 364 (Chronica Botanica, 1949–50).
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FORTHCOMING EVENTS

*(Meetings marked with an asterisk * are open to the public)*

Monday, May 11

UNIVERSITY OF LONDON (at the Royal Veterinary College, Royal College Street, London, N.W.1), at 5 p.m.—Dr. G. C. Ainsworth: "The Fungus".* (First of three Special University Lectures on Fungous Diseases of Animals". Further lectures on May 12 and 14.)

UNIVERSITY OF LONDON (at the Wright-Fleming Institute of Microbiology, St. Mary's Hospital Medical School, London, W.2), at 5 p.m.—Prof. G. W. Beadle (California Institute of Technology): "The Molecular Basis of Heredity".* (Last of a series of four Almroth Wright Lectures.)

UNIVERSITY OF LONDON (in the Physiology Theatre, University College, Gower Street, London, W.C.1), at 5 p.m.—Dr. J. B. Neilands (California): "Iron Metabolism in Micro-organisms".*

BRITISH SOCIETY FOR THE PHILOSOPHY OF SCIENCE (in the Joint Staff Common Room, University College, Gower Street, London, W.C.1), at 5.30 p.m.—Mr. G. Gorer: "The Concept of National Character".

ROYAL INSTITUTION, LIBRARY CIRCLE (at 21 Albemarle Street, London, W.1), at 5.30 p.m.—Dr. H. Lowery: "Gilbert White and His 'Natural History of Selbourne' ".

INSTITUTION OF ELECTRICAL ENGINEERS, EDUCATION DISCUSSION CIRCLE (at Savoy Place, London, W.C.2), at 6 p.m.—Discussion on "Trends in the Teaching of Electric Machine Theory in the United States", opened by Mr. P. L. Alger.

ROYAL GEOGRAPHICAL SOCIETY (at 1 Kensington Gore, London, S.W.7), at 8.30 p.m.—Mr. Eric Shipton, C.B.E.: "Explorations in Patagonia".

Monday, May 11—Wednesday, May 13

INSTITUTION OF CHEMICAL ENGINEERS; SOCIETY OF INSTRUMENT TECHNOLOGY; and the BRITISH COMPUTER SOCIETY (at the Central Hall, Westminster, London, S.W.1)—Joint Symposium on "Instrumentation and Computation in Process Development and Plant Design".

Tuesday, May 12

SOCIETY OF CHEMICAL INDUSTRY, AGRICULTURE GROUP (at 14 Belgrave Square, London, S.W.1), at 10.30 a.m.—Annual General Meeting, followed by a full-day Meeting on "The Manuring of Tropical Crops".

UNIVERSITY OF LONDON (at the London School of Economics and Political Science, Houghton Street, Aldwych, London, W.C.2), at 5 p.m.—Dr. Frederick Barth (Oslo): "The Anthropological Study of Middle Eastern Societies".*

UNIVERSITY OF LONDON (at the Royal Veterinary College, Royal College Street, London, N.W.1), at 5 p.m.—Dr. G. C. Ainsworth: "The Fungus and the Host".* (Second of three Special University Lectures of "Fungous Diseases of Animals". Further lecture on May 14.)

UNIVERSITY of LONDON (in the Physiology Lecture Theatre, Guy's Hospital Medical School, London Bridge, S.E.1), at 5 p.m.—Sir George Thomson, F.R.S.: "The Principles of Physics" (Fison Memorial Lecture).

ZOOLOGICAL SOCIETY OF LONDON (at the Zoological Gardens, Regent's Park, London, N.W.1), at 5 p.m.—Scientific Papers.

PARLIAMENTARY AND SCIENTIFIC COMMITTEE, GENERAL COMMITTEE (in the Grand Committee Room, Westminster Hall, House of Commons, S.W.1), at 5.30 p.m.—Discussion on "Modern Techniques and their Application to and Influence on New Buildings" opened by Mr. Norman Wates and Mr. Anthony Pott.

SOCIETY OF CHEMICAL INDUSTRY, CHEMICAL ENGINEERING GROUP (at 14 Belgrave Square, London, S.W.1), at 6 p.m.—Mr. W. Quick: "Fuel Economy in Chemical Works".

UNIVERSITY OF LONDON (at the Institute of Archæology, Inner Circle, Regent's Park, London, N.W.1), at 6 p.m.—Prof. J. D. Evans: "The Animal World of Prehistoric Farmers and Pastoralists in Europe".* (Second of six lectures on "Animals in Archæology". Further lectures on May 19, 26, June 2 and 9.)

BRITISH COMPUTER SOCIETY (at the Northampton College of Advanced Technology, St. John Street, London, E.C.1), at 6.15 p.m.—Mr. T. C. Hickman: "Early Experience with an E. D. P. Installation".

PHARMACEUTICAL SOCIETY OF GREAT BRITAIN (at 17 Bloomsbury Square, London, W.C.1), at 7.30 p.m.—Mr. N. J. Van Abbe: "Pharmaceutical Aspects of Dermatological Treatment".

Wednesday, May 13

UNIVERSITY OF LONDON (at the Postgraduate Medical School of London, Ducane Road, London, W.12), at 2 p.m.—Dr. E. F. Gale: "Disorganization of the Bacterial Cell by Antibiotics".*

ROYAL METEOROLOGICAL SOCIETY (at 49 Cromwell Road, London, S.W.7), from 2.30 p.m.–6 p.m.—Discussion on "The Dynamics of Cumulonimbus". Opening Speakers: Dr. R. S. Scorer, Mr. P. M. Saunders, Mr. F. H. Ludlam and Mr. W. G. Harper.

GEOLOGICAL SOCIETY OF LONDON (at Burlington House, Piccadilly, London, W.1), at 5 p.m.—Dr. F. Dixey: "The Geomorphology of Madagascar".

LINNEAN SOCIETY OF LONDON (at Burlington House, Piccadilly, London, W.1), at 5 p.m.—Prof. Hans Helbaek (Copenhagen): "Archæology and the Evolution of the Cultivated Plants, with special reference to *Linum*".

UNIVERSITY OF LONDON (at the Institute of Diseases of the Chest, Brompton Hospital, London, S.W.3), at 5 p.m.—Dr. R. W. Riddell: "Aspergilli as a Cause of Pulmonary Disorders".*

INSTITUTION OF ELECTRICAL ENGINEERS, RADIO AND TELECOMMUNICATION SECTION (at Savoy Place, London, W.C.2), at 5.30 p.m.—Mr. E. G. Rowe: "The Application of Statistical Techniques to the Electric Valve Industry".

SOCIETY OF CHEMICAL INDUSTRY, FOOD GROUP (at 14 Belgrave Square, London, S.W.1), at 6 p.m.—Scientific Papers (Seventh Young Members' Meeting).

SOCIETY OF INSTRUMENT TECHNOLOGY, DATA PROCESSING SECTION (at Manson House, Portland Place, London, W.1), at 6 p.m.—Annual General Meeting, followed by Mr. J. R. Sturgeon: "A Multipoint Digital Strain-Gauge Recorder".

BRITISH INSTITUTION OF RADIO ENGINEERS (at the London School of Hygiene and Tropical Medicine, Keppel Street, Gower Street, London, W.C.1), at 6.30 p.m.—Prof. D. Gabor, F.R.S.: "Improving Communication Techniques—What Have Engineers to Learn from Information Theory ?".

ROYAL INSTITUTION (at 21 Albemarle Street, London, W.1), at 9 p.m.—Dr. G. Kitson Clark: "The Modern State and Modern Society: Historic Tendencies and Future Probabilities".

Thursday, May 14

ASSOCIATION OF AGRICULTURE (at 53 Victoria Street, London, S.W.1), at 11.30 a.m.—Annual General Meeting.

UNIVERSITY OF LONDON (at the Royal Veterinary College, Royal College Street, London, N.W.1), at 5 p.m.—Dr. G. C. Ainsworth: "The Fungus, the Host, and the Environment".* (Last of three Special University Lectures on "Fungous Diseases of Animals".)

INSTITUTION OF ELECTRICAL ENGINEERS (at Savoy Place, London, W.C.2), at 5.30 p.m.—Annual General Meeting. 6.30 p.m.—Prof. E. C. Cherry: "On the Conceivable Future of Telecommunications".

ROYAL AERONAUTICAL SOCIETY (at the Institution of Mechanical Engineers, Birdcage Walk, London, S.W.1), at 6 p.m.—Mr. C. J. McCarthy: 47th Wilbur Wright Memorial Lecture.

INSTITUTE OF BIOLOGY (at Brunel College of Technology, Woodlands Avenue, Acton, London, W.3), at 7.30 p.m.—Dr. L. G. C. E. Pugh: "Men at High Altitudes".*

Friday, May 15

INSTITUTE OF NAVIGATION (at the Royal Geographical Society, 1 Kensington Gore, London, S.W.7), at 5.15 p.m.—Captain E. G. Irving, O.B.E., R.N.: "Electronic Surveys in the Caribbean".

SOCIETY OF CHEMICAL INDUSTRY, FINE CHEMICALS GROUP (at 14 Belgrave Square, London, S.W.1), at 6.30 p.m.—Annual General Meeting. 7 p.m.—Dr. H. T. Openshaw: "New Developments in the Chemistry of Emetine and Related Alkaloids".

APPOINTMENTS VACANT

APPLICATIONS are invited for the following appointments on or before the dates mentioned:

ASSISTANT LIBRARIAN (with a good honours degree and experience in an academic library)—The Registrar, Queen Mary College (University of London), Mile End Road, London, E.1 (May 16).

LECTURER and an ASSISTANT LECTURER IN THE DEPARTMENT OF PHYSICS—The Registrar, The University, Nottingham (May 16).

LECTURER or ASSISTANT LECTURER (with a good honours degree in physics) IN PHYSICS—The Dean, Guy's Hospital Medical School, London Bridge, London, S.E.1 (May 21).

ASSISTANT LECTURER IN GEOGRAPHY—The Registrar, The University, Sheffield (May 23).

ASSISTANT LECTURER or LECTURER (preferably with interests in applied mathematics) IN THE DEPARTMENT OF MATHEMATICS; and a TEMPORARY ASSISTANT LECTURER or TEMPORARY LECTURER (preferably with interests in applied mathematics) IN THE DEPARTMENT OF MATHEMATICS—The Secretary, University College, Gower Street, London, W.C.1 (June 1).

LECTURER (preferably with an interest and experience in the field of electrical machinery) IN ELECTRICAL ENGINEERING—The Deputy Registrar, The University, Birmingham 15 (June 1).

ASSISTANT EXPERIMENTAL OFFICER (with a degree in a biological subject and practical experience in bacteriological methods and techniques) for work on the ecology of soil micro-organisms—The Secretary, Rothamsted Experimental Station, Harpenden, Herts (June 30).

SENIOR LECTURER IN MATHEMATICS at the University of Melbourne, Australia—The Secretary, Association of Universities of the British Commonwealth, 36 Gordon Square, London, W.C.1 (Australia, (June 30).

CHAIR OF ENGINEERING—The Registrar, The University, Leicester.

SENIOR LECTURER (with good academic qualifications, teaching experience and preferably special qualifications or experience in mathematical statistics or other branch of applied mathematics) IN MATHEMATICS—The Registrar, Hatfield Technical College, Hatfield.

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